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THE AMERICAN JOURNAL OF PSYCHOLOGY

Early Publication of Papers

To the Contributors:

In spite of a rigorous selection of papers The American Journal of Psychology usually has in hand sufficient copy to fill a complete volume. Since publication follows the order in which manuscripts are accepted, papers may appear, under present conditions, nine months to a year after they are received from the author. This is unfortunate both for the contributor and for the Journal.

As a means of avoiding this long delay, the editors have frequently been urged to publish two volumes a year, or to establish a monographic series. While either plan would relieve the situation, the editors believe that a greater financial burden should not be placed upon the subscribers.

The editors have therefore tentatively adopted, as an alternative, a plan that has been successfully used for more than five years by the *American Journal of Botany*. In accordance with this plan, contributors may secure early publication of their articles by assuming the cost of their publication. This plan will, in effect, increase the size of the Journal at no additional expense to the subscribers, as these articles will be published *in addition* to the pages of each quarterly issue (156) to which the editors are committed.

The plan will not delay the appearance of other papers. On the contrary it may, in point of fact, insure their earlier publication. For example, an author, whose paper is scheduled to appear in the July number, may have it published, under this plan, in the preceding January. The place of his paper in the July number would then be filled by one previously designed for the October number.

The following items enter into the cost of publication: (1) composition; (2) plate making (cuts and half-tones); (3) correction of proof; (4) press-work; (5) paper; (6) binding; (7) postage and mailing costs; and (8) offprinting (paper, press-work and binding). Under the plan now proposed a contributor may, by assuming the costs of items (1), (2), (4) and (5)—the other items being absorbed by the Journal—have an article published in the first issue of the Journal made up after its acceptance.

It is thought that institutions may be willing at times to meet the expense involved in this arrangement in order to secure the early publication of papers for the members of their faculties or staffs, and that individuals will appreciate the opportunity thus offered of having graduate theses and other important papers brought out promptly.

Since every article published in the Journal carries its date of acceptance, the question of priority is clearly established.

Papers submitted to the editors for publication under this plan will receive the same critical scrutiny as all others.

Estimates of the cost for publication under the plan outlined here may be obtained from any one of the editors.

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THE EFFECT OF PRACTICE UPON THE UPPER LIMEN FOR TONAL DISCRIMINATION

By John F. Humes, University of Pennsylvania

It has been shown by Cameron that practice is one of the factors involved in the improved discrimination of vocalized tones.[1] Gough has also demonstrated that "practice has a marked effect on an individual's ability to identify musical notes that is distinct from the initial capacity of the individual or from the type of instrument used in producing the notes."[2] These results suggest that practice might increase the sensitivity of hearing to such an extent as to affect the upper tonal limen.

The purpose of this study was, therefore, to discover whether practice affected the upper terminal limen for tone, and, if so, how.[3]

Method and Procedure

A sound-proof room was not desired as silence and freedom from distraction produce in the *Os* a critical attitude toward the stimulus,[4] and as minor distractions have been found to increase accuracy of judgment.[5]

*Accepted for publication April 20, 1929.

[1] E. H. Cameron, Effects of practice in the discrimination and singing of tones, *Psychol. Monog.*, 23, 1917, (no. 100), 179.

[2] Evelyn Gough, The effects of practice on judgments of absolute pitch, *Arch. Psychol.*, 47, 1922, 86.

[3] A series of preliminary observations was made in November and December, 1926, the object of which was to choose suitable *Os* as well as to find a room which would meet the requirements of the experiment. The only room available at that time was an unoccupied office on the third floor of Bennett Hall. In March 1927, a room which was more adapted to the needs of the problem was procured in the Medical Laboratory. Here the investigation progressed until June when it was interrupted by the close of the term. This room was not available in the fall, consequently the experiment was not resumed until December, 1927, at which time a room was obtained, through the courtesy of Dr. R. Tait McKensie, in the Hutchinson Gymnasium of the University. These changes produced no apparent or measurable effect in the results.

[4] D. A. Anderson, Duration of tones, the time interval, the direction of sound, darkness and quiet, and the order of stimuli in pitch discrimination, *Psychol. Monog.*, 16, 1914, (no. 69), 155.

[5] F. O. Smith, The effects of training in pitch discrimination, *Psychol. Monog.*, 16, 1914, (no. 69), 92.

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Apparatus. A set of 40 steel cylinders of the Koenig type,[6] ranging from 16,200 to 24,000 d.v. by increments of 200 d.v., were employed.[7] The apparatus used in actuating the cylinders is fully described and pictured by Fernberger,[8] hence its description is omitted here.

The apparatus, it must be said however, eliminated two disturbing sources of error—variations in intensity[9] and in localization.[10] The cylinders were actuated by a falling steel ball.[11] Six balls of uniform weight were used in chance order during the course of an experimental hour. Table I gives the weights, distance of fall, and the resultant energy of the balls at the moment of impact.[12] The accuracy of the measurement of the weights is ±0.0002 grm., and of the distance of fall, ±0.10 cm. The energy of the ball at the moment of impact is the product of the weight of the ball by the distance through which it falls. For the purpose of this experiment we may neglect the slight variation in the third column of Table I and consider the factor of intensity as practically constant.[13]

TABLE I

SHOWING THE WEIGHT OF THE DIFFERENT BALLS, THE DISTANCE OF FALL, AND THE ENERGY OF IMPACT
(Distance of fall 20 cm.)

Ball No.	Weight (grm.)	Energy (grm. x cm.)
1	8.205	164.10
2	8.208	164.16
3	8.213	164.26
4	8.222	164.44
5	8.223	164.46
6	8.224	164.48

Observers. The *O*s, all members of the staff of the Department of Psychology, were Carl Altmaier, Jr. (*A*), F. R. Gillespie (*G*), Francis Irwin (*I*), Dr. Miles Murphy (*M*), Sidney Sanderson (*Sa*), and E. M. Twitmyer (*T*).[14] Their ages ranged between 24 and 35 years. While none of the *O*s was, in the strictest sense, highly trained in psychophysical observation, *Sa* and *M* were more

[6] Manufactured by the Standard Scientific Co., of New York.

[7] Cf. E. G. Boring, The construction and calibration of Koenig cylinders, this JOURNAL, 38, 1927, 125-127.

[8] S. W. Fernberger, An improved apparatus for actuating tonal cylinders of the Koenig type, *ibid.*, 38, 1927, 120-124.

[9] Cf. E. F. Möller, The effect of change of intensity upon the upper limit of hearing, *ibid.*, 33, 1922, 570-577.

[10] Fernberger, *op. cit.*, 120.

[11] *Ibid.*, 123.

[12] Cf. E. W. Scripture and H. W. Smith, Experiments on the highest audible tone, *Yale Studies*, 2, 1894, 105.

[13] C. C. Pratt (Highest audible tones from steel cylinders, this JOURNAL, 31, 1920, 406) found that slight variations in intensity did not significantly alter the relative frequencies of judgments.

[14] I am grateful to the *O*s for their coöperation which extended over a protracted period. I wish also to acknowledge my indebtedness to Professor Fernberger, who suggested the problem and assisted me throughout its course; to Mr. M. R. Wehr, who made the weighings of the steel balls in the Physics Laboratory; and to Mr. John R. Roberts, who supervised the care of the apparatus.

experienced than the others, all of whom had had but slight acquaintance with this type of work. *Sa* and *M*, some years before this experiment was begun, had made several observations on the same material used in the present study; but it was assumed, and subsequent observation bore out the assumption, that the previous experience would exert no influence on this experiment. No indication as to the purpose of the study was made to any of the *Os*. *Sa* and *A* observed during the preliminary series only, for reasons later to be described.

Procedure. In some respects the method follows that used by Möller.[15] *O* was seated with his back toward the apparatus, at a distance of 4 ft. from the center of the cylinder being actuated. The relation of the position of *O* to the arrangement of the apparatus was such that the actuated cylinder was always in the median plane of *O*'s head. The following instructions were read at the beginning of each period of observation: "You will experience a number of auditory sensations. In each case you will immediately report whether the experience was tone or not tone; if tone say, 'Yes;' if not tone say, 'No'."

No further instructions were given. A preparatory signal, 'Ready, now,' preceded the actuation of each cylinder by about 2 sec. Observations were recorded immediately. Since it was desired to study the effects of practice, each response was considered important and consequently recorded. Frequently an *O* would request that a stimulus be repeated, invariably because of doubt as to judgment. Repetition was permitted as often as requested.

The *Os* varied considerably in the length of time required to determine a critical stimulus. When this was once determined, it was made the central stimulus of a series of seven, three of which were the next in order above, and the other three the next in order below that point. The cylinders were then arranged on the apparatus so that the presentation of the stimulus would be in haphazard order. Fifty judgments were made on each cylinder, 350 in all, at every session, for the purpose of calculating a statistical limen. In several instances, however, the number of judgments was greater or less than that just specified, due to difficulty in determining a critical stimulus. Rest periods of from 1-3 min. were permitted after every 100 judgments. Every observational session consumed about an hour.

The psychometric function that determines the upper tonal limen was calculated from the observed relative frequencies of affirmative judgments in accordance with the phi-gamma hypothesis. Urban's Tables[16] were used to simplify the calculations, which are recapitulated in Table II. This table shows the numerical value of the statistical thresholds. The limens (L) and the indices of precision (h) were computed by linear interpolation between successive values.[17]

At the completion of each session the *Os* were asked to write an introspective report, including a statement of the criteria upon which their judgments were based.

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[15]Möller, *op. cit.*, 572.

[16]F. M. Urban, Hilfstabellen für die Konstanzmethode, *Arch. f. d. ges. Psychol.*, 24, 1912, 236-243.

[17]Cf. S. W. Fernberger, A preliminary study of the range of visual apprehension, this JOURNAL, 32, 1921, 123. Also E. G. Boring, Urban's tables and the method of constant stimuli, *ibid.*, 28, 1917, 282.

Results

Statistical limens (L) of tonal quality, measured in terms of frequency of vibration, and coefficients of precision (h) are presented in Table II for every *O*, for every series of experiments.

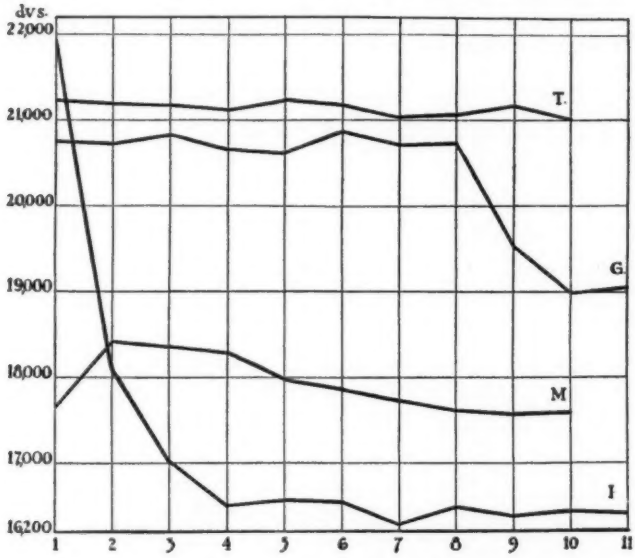

Fig. 1. Showing The Effect Of Practice Upon The Upper Limen Of Tonal Discrimination

Eleven calculations were made for *G* and *I*, and 10 were made for *T* and *M*. It should be borne in mind that these values, because the vibration rates of the tonal cylinders were calculated, have relative rather than absolute validity.[18]

It was expected that practice would increase the magnitude of the upper terminal limen. An inspection of Table II reveals, however, no such result. On the contrary, there is, in the case of every *O*, a noticeable decrease in this value. The higher frequencies, at first experienced as 'tone,' are no longer experienced as such.

[18]Boring, *op. cit.*, 125.

TABLE II

SHOWING THE TONAL LIMEN (L) AND MEASURE OF PRECISION (h) FOR EVERY *O* FOR EVERY SERIES OF EXPERIMENTS

Series	*G*		*I*		*T*		*M*	
	(L)	(h)	(L)	(h)	(L)	(h)	(L)	(h)
I	20750	0.0023	21942	0.0012	21202	0.0019	17641	0.002
II	20741	0.0037	18178	0.0013	21177	0.0022	18402	0.0011
III	20802	0.0026	17056	0.0021	21174	0.003	18359	0.0013
IV	20680	0.0017	16530	0.0013	21124	0.002	18275	0.0018
V	20623	0.0026	16584	0.0016	21251	0.0026	17912	0.0017
VI	20855	0.0022	16569	0.0016	21171	0.0023	17863	0.0024
VII	20706	0.0034	16267	0.0012	21055	0.0021	17754	0.0024
VIII	20745	0.0034	16496	0.0016	21061	0.0023	17668	0.0025
IX	19527	0.0022	16393	0.0018	21167	0.002	17646	0.0016
X	18922	0.0029	16432	0.0011	21019	0.002	17659	0.0024
XI	19056	0.0028	16420	0.0012				

The limens. An examination of Fig. 1, which pictures diagrammatically the results shown in Table II, reveals marked individual differences in the upper limen. There are four well-defined levels, and but one overlapping.

I's limen in Series I is well above that of any of the other *Os*. In Series II, however, his limen drops to a point far below that of *T* or *G* and slightly below that of *M*; and in Series III it drops still further to a point where it is much lower than that of any of the other *Os*. In Series IV, *I*'s limen reaches a point of relative stability. Thus *I* starts out with the highest terminal limen and ends with the lowest. It should be noted here that 9 weeks elapsed between *I*'s first and second series. During that time he observed at 6 different experimental sessions, the results of none of which were sufficiently homogeneous to admit of calculation. Series II was the last one calculated before the close of the regular school year. Between Series II and III there was another interruption of about 9 months. During the latter part of this period (*i.e.* from the first of December, 1927—when work was resumed—to February 23, 1928) *I* observed at 5 sessions the results of which were again so irregular that no limens could be calculated. Series III was obtained during the latter part of February, 1928, Series IV 5 days later; and from then on the different series were obtained at weekly intervals.

Two interruptions occurred in *G*'s work: one between June and December of 1927; and the other between the latter part of December and the middle of the following February when the holidays and mid-term examinations interrupted the work. These interruptions are not reflected in his results. The drop in his limen in Series IX, which was the first value calculated after work was resumed in February, was due, not to the interruption, but to inspissated cerumen of his left ear. This troubled him for over a month. Series X, obtained one week after Series IX, shows a further drop; but Series XI, obtained after an interval of another week, shows a slight rise. Observations obtained still a week later were so irregular that a limen could not be calculated.

Except in the case of *M*, the values as determined at the beginning of the study, are all higher than at the end. The first limen of *M* turns out to be his lowest. The highest value determined for him was in Series II. A steady, though gradual, descent in the curve from this point is apparent up to Series X, which is but slightly higher than the one previously calculated. Successive periods of observation occurred closer together, in point of time, for *M* than for any of the other *Os*, the ten series having been made within two months (April 18 to June 14, 1928). One set of observations, made between Series IV and V, could not be statistically treated.

The curve of *T*, aside from one inversion, which is not strikingly significant, is the most stable of all. While there is a fairly gradual decrease in the liminal values, there is no significant variation except in Series V. For *T*, continuity of observational sessions was interrupted three times. The first limen was determined about the middle of April, 1927; the second, just previous to the December holiday. A lapse of 2 mo. occurred between Series III and IV; and

4 weeks intervened between Series IX and X. These interruptions were due to vacation periods and to *T*'s inability to serve in the experiments. The results plainly indicate that the intermissions were inconsequential. The observations of *T* were uniform throughout, so that in no instance was it necessary to discard any of his results.

Coefficients of precision. Tables of observed relative frequencies are omitted, because of their bulk, but an idea of the comparative steepness of the curves may be gained by considering the coefficients of precision (h). These have been averaged and the mean variation determined for every *O*. The values are as follows: *G*, 0.0027 ±0.0005; *T*, 0.0022 ±0.0002; *M*, 0.0019 ±0.0004; and *I*, 0.0014 ±0.0003. It will be seen from these results that *G* has the highest average h and the greatest m.v.; that *T*'s average h ranks second and his m.v. fourth; that *M*'s average h and m.v. are both in third position; and that *I*'s average h ranks fourth and his m.v. second. On the basis of these results no generalization may be advanced.

The critical stimulus. The value of the critical stimulus varied in the different series for every *O* except *T*.

For *T* the cylinder of 21,200 d.v. was used in all the series. For *I*, however, the cylinder of 22,200 d.v. was used in Series I, of 18,400 d.v. in Series II, of 17,200 d.v. in Series III, and of 16,800 d.v. in Series IV-XI. The cylinder of 21,000 d.v. was used in the first eight series for *G*, but thereafter, because of the condition of the ear referred to above, values of 19,600, 19,000, and 19,200 d.v. were used respectively in Series IX, X, and XI. The values of the critical stimulus used for *M* varied as follows; in the first three series the cylinder of 18,4000 d.v. was used, in the next four series (Series IV-VII) the cylinder of 18,000 d.v. was used, and in the last three series (Series VIII-X) the cylinder of 17,800 d.v. was used.

Numerous attempts were made over a period of 4 mo. to determine a critical stimulus for *A* and *Sa* but without success. These *O*s were tested over the entire range of the apparatus and their reports were so irregular that a limen could not be calculated from them. It is interesting to note, however, that the majority of their reports were positive even in the upper regions of the scale. *Sa*, for example, on an occasion when the cylinders varying from 22,800 to 24,000 d.v. were used, reported "this whole series seemed to be tone, and at no time was I in doubt of my judgment." Of the 70 judgments returned with this series, 63 were 'yes.' There was, however, *Sa* added, "little discriminable difference between the tones."

It was assumed that neither of these *O*s was able to distinguish in this range between tonal and non-tonal qualities and further work with them was therefore abandoned. It was later learned that they both were 'tone deaf,' in the sense that neither could 'carry a tune.'

Discussion of Results

The decrease in the upper terminal limen is due probably to a change in a number of factors, but prominent among them is the criteria employed by the *O*s in their judgments of tone.

The introspections reveal that the subjective standard of judgment changed as the experiment progressed. Thus while no improvement in sensory acuity was noted, the *O*s did develop a higher degree of discrimination in their judgments of a tone. Every *O* became more conservative and critical in his judgments as the experiments progressed.

As Möller has pointed out,[19] a statement of the limen only gives a little of the available information. Adequate interpretation requires an analytic treatment of the results. For the purpose of this study it has been deemed unnecessary to deal with every individual limen, but it is important to examine the introspections since they shed light upon the general aspects of the problem.

(*1*) *Criteria for tone.* Since no attempt was made, in giving the *O*s their instructions, to dictate the criteria of tone, equivocality is perhaps to be expected. We found that the criteria of judgment varied in the early stages of the experiment, not only from series to series, but even during the course of a single period of observation. *M*, for example, reported in Series II:

"The formation and maintenance of a criterion seemed more difficult this session. The experience was very similar to one I have had in grading examination papers, in which I have at times wondered whether my standard has not changed."

There is, however, more or less general agreement among the *O*s as to the characteristic qualities of the stimuli. A sound of a 'ringing' nature is judged as 'tone,' while a 'click' or a 'thud' is described as 'no tone.'

I, for example, said, "I distinguish a tone by its ringing quality. Whenever I hear a twang or a sharpening, I judge it a tone; when I hear just the click of the ball hitting the cylinder, I judge it not a tone." This is of particular interest since it was written after Series I, which is the highest limen calculated in the entire experiment, Thereafter it required stimuli of considerably fewer vibrations to be characterized as 'tone' on the basis of the 'ringing.' This result can be explained, we believe, in no other way than by a shift of attitude.

T, according to his reports and the observed relative frequencies of judgments, maintained a constant attitude throughout the experiment. He writes:

[19] Möller, *op. cit.*, 576.

"I seemed to recognize two series of auditory stimuli. One had the content of a thud—this I called 'no tone.' The second type of stimulus carried with it a sort of 'ring'—this I called 'tone'."

G similarly characterizes 'tone' by "a ring which lasts but a very short time after the blow. If the ball strikes the cylinder with a thud, it is merely a noise. In many cases there is just the slightest suspicion of the ring, yet not enough to be classed as a tone."

M uses the same terms. He says, " 'Tone' is something added to the experience. 'No tone' is a dull thud. 'Tone' is that plus something more. The 'something more' is characterized by duration and a kind of 'ring'."

A and *Sa* use similar criteria in differentiating the experiences. *A* says, "The difference between a tone and not-a-tone is determined largely by the presence or absence of a thud. Whenever I hear the dull thud I judge 'no tone.' The absence of the thud determines 'tone,' but there seems also, in the case of 'tone' to be a 'ring'." *Sa* reports that "a metallic clang with an after-ring seems to be my criterion for tone. 'No tone' is a 'flat' sensation of short duration, lacking the after-ring."

The 'ring' and the 'thud' remain the basic differentia during the entire experiment. But the specific location in the scale where these perceptions occur is by no means constant. This variability is undoubtedly a function of the stimulus attitude.

(*2*) *Intensity.* The objective variations in intensity were, as shown in Table I, negligible, yet the *O*s reported intensive differences in their experiences.

I writes, after Series X, "I particularly noticed a phenomenon which I have noticed before, that the intensity of the sound seem to vary considerably. By intensity I mean the series of experiences which vary from loud to soft which are expressed in music by *forte* and *piano*. The 'tones' seem in general to be louder than the 'no tones,' though I cannot make an accurate statement about this relation."

M (Series VII): "I detected very great differences in intensity, apparently between various stimuli which are judged tone. In some the tonal intensity seems very high, in others faint."

G (Series VII): "Occasionally one of the tone sensations would ring out with more intensity than any of the others before. On the other hand, some of the 'no tones' would come as a gentle rap, entirely unlike any other 'no tones'."

A: "I noticed today that there was a louder tone sensation than I had heard before."

Neither *T* nor *Sa* report intensity differences.

Since the reports were made at the close of an experimental period, it was impossible to identify the particular stimulus that produced a judgment of greater intensity. Occasionally, however, an *O* would voluntarily indicate after the reception of a stimulus, an intensity difference. A repetition of several stimuli (including the one judged more intense) never resulted in the per-

ception of a difference. We feel warranted, therefore, in ascribing the intensive differences reported to subjective factors.

(3) *Auditory after-effects.* After-experiences were reported by some of our *O*s, as shown by the following excerpts from the introspections.

T (Series I): "[One] type of stimulus carried with it a sort of ring, or what seemed to be a momentary continuation of the initial impact. I think of it as an after-effect as distinguished from the abruptness of the first type ('no-tone'). This I called tone, for it seemed to carry more than the initial impact."

M (Series I): "Tone seems longer in duration than 'not tone.' The 'not tone' is a click or a thud which is brief and quickly gone. Tone 're-echoes,' as it were. Whether these echoes which seem to add to the duration of the sensory experience are aspects of the sensation primarily, or after-images, I am not able to say." After Series X, he writes, "the presence of pitch seems to carry with it a gradual, uniform falling away of sound."

Sa: "The tonal sensation always lasted longer; it is a ringing sensation followed by an echo-like effect."

Obviously differentiation of tone and not tone depends, to a large extent, upon the relative duration of the perception. The writer inclines to the belief, however, that the 'after-experiences' reported by the *O*s are not after-images analogous to the after-images of vision, but that they are either Bishop's 'modified endings,'[20] or experiences derived from the prolonged vibration of the stimulating media, or memory after-images. The results are not at hand to enable us to decide which of these is the true explanation.

(4) *Assurance and doubt.* The *O*s were not instructed to report the degree of certainty with which judgments were made. Consequently, subjective assurance is not indicated in definitely quantitative terms. Nevertheless, it seems certain, from the references made by the *O*s to the "difficulty of the task," to the fact that they "were guessing at first," to the "feeling of uncertainty" regarding their judgments, and from the numerous requests for a repetition of the stimuli, that the difficulty experienced by the *O*s in establishing constant criteria was in a large measure due to uncertainty. This is in agreement with Boring's statement that "doubt is the most constant offender" in the control of attitude.[21]

[20] H. G. Bishop, An experimental investigation of the positive after-image in audition, this JOURNAL, 32, 1921, 325.

[21] E. G. Boring, The control of attitude in psychophysical experiments, *Psychol. Rev.*, 27, 1920, 445.

(5) *Expectation and comparison.* The *O*s made a few references in their protocols to expectation[22] and comparison, consequently the extent to which these factors influenced their judgments cannot be definitely established. It is probable, however, that both of these factors were frequently effective. The occasions are chiefly due, we believe, to the chance arrangement of the stimuli in the stimulus-series,[23] therefore, whenever *E* suspected that either was effective, the order of presenting the stimuli was changed.[24]

The tendency to compare a present experience with one immediately preceding it was reported by several *O*s, as the following excerpts illustrate.

G (Series X): "The alternation of 'tone' and 'no-tone' became so evident that I unconsciously pre-perceived what was coming next."

M (Series II): "The sequence seemed important this time. I believe, if a sensation follows one which is positively 'tone,' that it is less likely to be judged 'tone' than if preceded by a doubtful experience. In other words, the judgment is influenced by the preceding experience."

A: "I felt that my judgment was somewhat influenced by the previous one. If a clear tone was heard, then the next one would invariably be a dull thud, which might have been judged a tone had it been isolated in experience."

These reports accord with Fernberger's conclusions in his experiments with lifted-weights,[25] that a stimulus is as likely to be judged one way as another, depending upon its position in the series.

(6) *Affection.* Three of the *O*s, as the following excerpts show, refer definitely to affective accompaniments of the sensory experiences.

I (Series VIII): "I noticed an affective reaction to the auditory sensations. When a sensation is surely 'tone' it is very pleasant; in doubtful cases there is unpleasantness. The greater the difficulty in reaching a judgment, the greater in general the unpleasantness. A sensation which is surely 'not-tone' is also pleasant, though less intensely so than in the case of one that is surely tone."

Sa (in his first report): "Tone is marked by an unpleasant tingling in my ears."

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[22] *Ibid.*, 445.

[23] Cf. S. W. Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 3, 1920, 132.

[24] The effect of expectation in pitch discrimination was noted by F. O. Smith (*op. cit.*, 99) who says, "with a strong expectation of hearing the second tone high or low, the organism is set to make the appropriate response and this has a marked influence upon his judgment."

[25] Fernberger, *op. cit.*, 149.

M (Series I): " 'Not-tone' is always more pleasant than 'tone.' The tones are all high and unpleasant, and at times they are almost painful."

The affective aspects of the experience depend, in the case of *I*, not upon the tonal quality, but upon the degree of assurance that attached to his judgments. In the cases of the other two *O*s, tone is either accompanied by unpleasant consequences, *i.e.* a tingling in the ears, or is itself unpleasant. These results corroborate those of Smith—high tones for some of our *O*s are intrinsically unpleasant.[26]

(7) *Attentional factors.* Four of the *O*s referred specifically to attention as a factor of importance in their judgments. Maximal attention was required in the experiments. Whenever attention began to wane or to waver the *O*s reported that their judgments became difficult and uncertain. They further reported that they resorted, in such instances, to various tricks, such as opening or closing their eyes, to sustain their attention. Whenever distractions occurred or *O*'s attention was poor the experiments were repeated or temporarily interrupted.[27]

(8) *Physiological conditions.* The *O*s reported varying states of health during the course of the experiments. With the exception of *G* none of them, however, suffered anything more than a 'cold in the head.' *G*, as has already been mentioned, suffered from an aural disorder of the left ear.

The disorder of *G*'s left ear was first noticed in Series IX when the critical stimulus (which had been constant at 21,000 d.v.) had to be changed to 19,600 d.v. Our original intention was, when we discovered this defect, to exclude the results and to hold up further observations until *G* had recovered. It was decided, however, to report them and to continue the experiments, because the results were considered to be of theoretical significance.

There are two possible explanations for the decrease in *G*'s sensitivity in Series IX, X, and XI. The impacted ear-wax may have lowered the intensity of the stimulus and this may have resulted in the lowering of the limen. This explanation stands, however, in opposition to the results of Möller's study in which she investigated the effect of changes of intensity upon the upper limit of hearing. She found that the upper tonal limen did not vary with intensive

[26]Smith, *op. cit.*, 87.

[27]It was necessary in a few isolated instances to suspend work because of adventitious sounds. The observations made during such periods were discarded.

differences.[28] The impacted ear-wax may, on the other hand, have resulted in the functional loss of *G*'s left ear and this may have resulted in the lowering of the limen. This explanation stands in line with the results of Knudsen[29] and Young.[30] Both of these investigators found that sensibility is greater when both ears function together than when either functions separately. That *G* was, during the last three series of experiments, observing monaurally, is borne out by one of his introspections. He said, after the experiments of Series X, "The condition of the left ear was worse than ever before. None of the stimuli were heard in that ear. After realizing this, attention was directed to the right ear."

It is not clear, however, why the monaural limen should be lower than the binaural. Even though it be granted that the intensity of the stimulus is less in monaural than in binaural hearing, intensity may not, in the light of Möller's results, be used to explain the difference.

I's health may have been an important factor in the early stages of the experiment. After several experimental periods when his results were particularly irregular, he reported feeling "not physically well." After Series III, when he was approaching a standard criterion which he later used throughout the experiment, he wrote, "Today I am feeling better than at most, perhaps all, of the other sessions."

M reported a 'cold' on a day when his observations were too erratic to be computed. *T* likewise reported a 'cold,' during Series VI, which he believed had affected his left ear; his results did not, however, bear that out as they were not noticeably affected.

While we cannot generalize, it appears from these results that the *O*s, in order to return the best possible reports, should be in a good state of physical health.

(*9*) *Other factors:* (*a*) *Kinaesthesis.* *I* was the only one of the regular *O*s to mention kinaesthesis. He described his experience of tone as being "more like a sharp thudding against the ear than an auditory sensation." Both *A* and *Sa* referred in the preliminary experiments to kinaesthetic experiences. *A* reported that "the tone was determined by a feeling of vibration . . . a greater fullness in my ears than in the case of 'no-tone'." *Sa* observed "an unpleasant tingling in my ears." And again that "at times there was a distinct kinaesthetic sensation inside the ear."

Just how much the awareness of muscular strain entered into the *O*'s judgment can not be decided here. The results offer no clue as to the effect of

[28]Möller, *op. cit.*, 577.

[29]V. O. Knudsen, The sensibility of the ear to small differences of intensity and frequency, *Phys. Rev.*, 19, 1923, 84-102.

[30]R. C. Young, Binaural *vs.* monaural sensibility of the human ear to small differences in frequency, this JOURNAL, 37, 1926, 328.

kinaesthesis, nor do the introspective reports provide us with adequate material for a conclusive statement. We are, however, of the opinion that kinaesthesis is of definite importance and that it should not be disregarded.

(*b*) *Imagery.* On the basis of the introspective reports, imagery is relatively unimportant in these experiments, yet it must have been involved in them. It must have served, for example, as the basis of the factors of expectation and comparison discussed above. While not clearly described as such by any of the *Os*, the comparison of a present auditory experience with a preceding one evidently took place in imaginal terms, *i.e.* the memory of a preceding tone persisted long enough to be compared with a present one. Furthermore, the auditory 'after-effects' described above, may have been, as we remarked, nothing more or less than memory after-images.

(*c*) *Localization.* Localization, as the term is used here, means the general region of *O's* body at which the sound 'registers.' *G*, for example, stated (Series I) that "consciousness of the sound seemed to be in back of the head." After the last series he made a rough diagram of his head, indicating the various regions where the different qualities of the auditory experience were localized. *A* localized the tones, similar to *G*, in his head, although the localization was probably made in kinaesthetic rather than visual terms since he referred to a "decidedly sharp vibration in my head."

(*d*) *Volume.* Volume was seldom referred to in the introspective reports.[21] *I* stated in two reports, however, that some tones seemed "fuller" than others. *G*, on one occasion, mentioned that one of the tones had greater volume than the others. *Sa* also noted a variation in volume. It is difficult to say, however, whether these references are to imaginal contents or to actual perceptions of differences in volume.

(*e*) *Synaesthesis.* *M* alone manifested synaesthetic phenomena. After Series I he made the following statement, "There are many visual synaesthetic experiences. Tone is a dark circle with rays of light streaming from it; not-tone does not possess these radiations."

Summary

A review of the foregoing introspections indicates that even in a situation where the *Aufgabe* is relatively simple, a great deal of complexity may exist. It is well to repeat that a mere statement of the qualitative limen is insufficient for a complete comprehension of the problem. A consideration of all the relevant material available is necessary for the adequate interpretation of responses to the stimuli here employed.

Individual differences are in general greater than the variations of the judgments of the individual *Os*. We find the various *Os* placing emphasis upon different attributes of the mental process that led to their judgments.

[21] Massiveness or extensity of tone is the meaning here implied.

G, for example, made nine references to 'attention,' the focalization of which was consciously made and extremely necessary for a clear-cut judgment. Localization and imagery seemed to be fused in the three references which he made to these factors. His physical condition was also of concern to him, particularly in the later series, when his left ear was impacted. He referred to this condition three times, and every time in connection with the difficulty experienced in discrimination of tonal quality. He made but one reference to kinaesthesis, volume, intensity, after-image, and comparison and expectation.

I, on the other hand, placed greatest importance upon the degree of subjective assurance which accompanied his judgments. He made nine specific references to this factor. In the early stages of the experiment his unstable physical condition was mentioned four times in the reports. It is believed that this fluctuation of *I*'s general health was largely responsible for the long period required to establish a critical stimulus.

It is of interest to note the change that took place during the experiment in his criteria of judgment since it is more marked in his case than in the other *O*s. After Series II (computed two months after Series I) he said, "Today I seemed to have changed my criterion of tone somewhat. There seemed to be three fairly distinct types of sounds; those clearly not tones, those clearly tones, and a sort of middle class which did not have the sharp twang of what I have been calling tones, but had a kind of bell-like ring. These I called tones." Nine months later, after Series III had been completed, he stated, "In judging today I made a conscious effort to stabilize my criterion of tone by deciding to answer 'Yes' to all cases where I thought the odds were even or better; to all others, I answered 'No.' I used this criterion consciously only in doubtful cases." The next series was calculated one week later, and the same criterion was reported. *I* maintained this standard of judgment for the rest of the experiment; and it will be noted that there is but slight variation in the psychometric function from this point on.

I referred twice to attention and once to affection as factors which aided in subjective assurance. Scattered through his reports are single allusions to volume, kinaesthesis, and intensity.

T's emphasis was upon the temporal differences between 'tone' and 'no-tone.' There was no departure from this criterion. One reference to a 'cold in the head' was all that broke the uniformity of his reports. The maintenance of so constant an attitude resulted in the most stable psychometric function of all the *O*s.

M referred to a number of factors contributing to his judgments, but no one of them was emphasized. After Series I he reported, " 'Tone' is differentiated from 'not-tone' by other modalities as much as by audition." From this it would seem that for him the total mental state was important at the time of a judgment. *M*'s criteria of tone varied more than *T*'s or *G*'s, and less than *I*'s. By deciding to assign 'doubtful cases' to the category of 'no-tone,' *M* made his criteria more rigid. He did this at Series IV. From then on there is a more constant decrease in the liminal value for him than for any of the other *O*s. *M* mentioned, in his introspective reports, attention, auditory after-effects, and synaesthesis each twice, and affection, imagery, comparison, physical condition, and intensity each once.

Conclusions

The following conclusions may be drawn from the results of this study.

(1) The effect of practice is to decrease the upper tonal limen.

(2) Within the limits of the psychometric function, the individual *O* becomes more discriminating of qualitative differences.

(3) Marked individual differences are apparent in the qualitative psychometric functions for these four *O*s. Differences are also evident in the rate and degree of decrease of liminal values.

(4) There is a tendency, once a criterion is rigidly established, for the threshold to remain approximately constant.

(5) Introspective data, which supplement the psychometric are necessary for a complete interpretation of the statistical limen.

(6) Practice does not consistently affect the coefficient of precision, although the trend is in general toward stabilization of that value.

(7) A radical change in the qualitative threshold is largely a function of change of attitude.

(8) For one *O*, an incipient pathology of the auditory mechanism results in a decided obtuseness of qualitative sensitivity.

(9) Qualitative judgment depends upon an integration of auditory attributes and complex mental processes, as well as upon structural constituents, *i.e.* perceptual and imaginal contents.

STUDIES IN AFFECTIVE PSYCHOLOGY[1]

By Paul Thomas Young, University of Illinois

VIII. The Scale of Values Method

The original aim of the present study was to work out a technique for testing affective reactions. We hoped to develop norms of like and dislike similar, perhaps, to the well-known word association norms of Kent and Rosanoff. Owing to difficulties with the method the original aim has not been attained.

The following paper describes the method used and some of the difficulties encountered.

(*A*) *Constancy of affective judgments to odors.*[2] *S* was presented with a scale of affective values and instructed to make judgments upon odors. Thirty-two chemically pure organic substances were selected as stimuli and in addition an empty bottle which had been cleansed and sterilized served as a control.[3]

The experiment was conducted in a black-walled dimly illuminated room. An elevator shaft, about 3 × 5 ft., connecting the room with the upper floor of the laboratory, served as a vent. A table was placed in the bottom of this shaft and two fans—one above and one below—kept a current of air moving slowly upward during the experiment. The arrangement practically freed the experimental room from stray odors, diffusing them through the upper laboratory. The bottles were placed on a table in the bottom of the shaft. When not experimenting the door of the shaft was closed thus keeping odors outside the experimental room.

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[1]Continuation of the series, this Journal, 38, 1927, 157-193; 40, 1928, 372-400.

[2]The experimental work upon which the present study is based was done in the spring of 1923 by Miss Minna Libman under the direction of the writer.

[3]Dr. C. S. Marvel of the chemistry department, University of Illinois, assisted in selecting odors which would be chemically constant for the period of the experiment. Chemical designations have been published by Bentley (M. Bentley, Qualitative resemblance among odors, *Psychol. Monog.*, 35, 1926, (no. 163), 146). In the present experiment Nos. 1 to 36 of Bentley's list have been used except the four marked with an asterisk (Nos. 19, 26, 33, 35). These four were discarded on the ground that cutaneous irritation was involved. An empty bottle used for purposes of control was substituted for one of these (No. 33).

The bottles were black and of uniform size. Each bottle and corresponding stopper was labeled with a number. The chance order used in the experiment was as follows: 1, 9, 17, 25, 33, 2, 10, 18, 34, 3, 11, 27, 36, 4, 12, 20, 28, 5, 13, 21, 6, 14, 7, 8, 16, 24, 32, 15, 23, 31, 22, 30, 29.

S was seated on a comfortable chair with his back towards the stimuli. He was given the following instruction:

"The experimenter will place beneath your nostrils a bottle of odorous substance. Take three deep breaths every time an odor is presented.

"Immediately and without reflection indicate how the experience felt. If your attitude was one of indifference, indicate by o. If pleasure was experienced, indicate by +, and if displeasure was experienced, indicate by −. Study carefully the scale of values on the accompanying sheet so that you may give the *degree* of feeling by one of these numbers."

On a separate sheet the following scale of values was placed before *S*:

+5 very great pleasure
+4 great pleasure
+3 pleasure
+2 slight pleasure
+1 very slight pleasure
0 indifference
−1 very slight displeasure
−2 slight displeasure
−3 displeasure
−4 great displeasure
−5 very great displeasure

The above scale is symmetrical and balanced. Before deciding upon this particular scale of values we considered some of the lengths and types of scale used in other investigations.[4]

Seventeen students in the writer's class in experimental psychology served as *S*s. Two other *S*s were used for some of the odors so the number of judgments per odor varies from 17 to 19. The *S*s were taken one at a time into the experimental room. After reading the instructions and studying the scale of values an odor was presented. Affective judgment was made and immediately recorded. There was no special warning signal since the approach of *E* served as adequate warning. The thirty-two odors and one empty bottle (No. 33) were presented in haphazard order with a pause of half a minute or more between successive presentations. During the actual smelling the *S*'s eyes were closed.

For purposes of illustration seven odors and the empty bottle have been selected. The following table shows the frequency with which each point on the scale was used by the group of *S*s.

[4]We previously used three degrees of P and three of U plus indifference. In a recent investigation Kenneth used two degrees of P and two of U. Conklin used six degrees of P and six of U plus indifference. P. T. Young, Constancy of affective judgment to odors, *J. Exper. Psychol.*, 6, 1923, 182; J. H. Kenneth, A few odor preferences and their constancy, *J. Exper. Psychol.*, 11, 1928, 56; also *Psychol. Monog.*, 37, 1927, (no. 171); E. S. Conklin, The scale of values method for studies in genetic psychology, *Univ. of Oregon Pub.*, 2, 1923 (no. 1).

The scale-of-values method has also been used in connection with the study of attention. L. R. Geissler, The measurement of attention, this JOURNAL, 20, 1909, 511; K. M. Dallenbach, The measurement of attention, *ibid.*, 24, 1913, 468; The measurement of attention in the field of cutaneous sensation, *ibid.*, 27, 1917, 444 ff.

Odor No.	−5	−4	−3	−2	−1	0	+1	+2	+3	+4	+5
27	5	5	5	2	1						
2	2	2	7	4	3	1					
17		4	3	2	3	4	1				
4		1	2	2	4	2	2	2	3	1	
33				1	2	12	3				
24					3	1	5	6	4		
30					2	1	7	3	4	2	
23						1	5	5	4	2	2

The odors have been arranged in a series from the most U (unpleasant) to the most P (pleasant). Every one of the other odors can find a place somewhere in this series. Some are wholly on the U side, others wholly on the P side, others in between. The empty bottle (No. 33) presented in the context of odors is the most indifferent of the series—and yet there are three judgments of P and three of U for it.

The result illustrates a well-known fact that some odors induce U, others induce P, whereas others are more or less indifferent. The spread or variability in judgments is not uniform. No. 4 with a wide spread has intentionally been placed next to No. 33 to show this contrast. From the statistical standpoint odors might be ranked in "degree of indifference" just as definitely as in degree of P or U. From this point of view the empty bottle used as a control is the most indifferent member of the series.

Next we desired to see whether the affective judgments would be constant from day to day. Two *S*s, a man (*A*) and a woman (*B*), made affective judgments for 12 successive experimental days. The odors, instructions, procedure, etc. were as above except that a new haphazard order of presentation was used every day to eliminate learning of the series, expectation, and to distribute by chance the effects of contrast. Two odors and the empty bottle may serve to illustrate the result.

		Successive Days											
S	Odor-No.	1	2	3	4	5	6	7	8	9	10	11	12
A	21	−1	−3	−2	−3	−3	−3	−3	−4	−2	−3	−3	−3
	29	0	+1	+1	+1	+1	+1	+1	+3	+3	0	+2	+3
	33	−2	0	0	−2	0	0	−2	−1	0	0	−1	0
B	13	+4	+5	+5	+4	+4	+3	+4	+4	+2	+5	+5	+5
	1	−3	−4	−5	−4	−4	−5	−4	−4	−5	−3	−5	−3
	33	0	0	0	−3	0	0	0	0	−1	0	0	0

Considering the series of judgments we find definite uniformity. Some are consistently P from day to day, some consistently U, some (not shown) vary about I (indifference).

The total experiment demonstrates the existence of statistical constancy in affective judgment to odors. The result is neither new[5] nor startling but it is theoretically important. The experiment shows that we are in the presence of a genuine problem even though, as we shall see, the method is not ideal.

(*B*) *Difficulties with the scale-of-values method.* (*1*) *Affective judgment equivocal.* The affective judgment is equivocal regarding the existence or non-existence of felt experience. Objects may be judged affectively with little regard to the felt pleasantness and unpleasantness of experience at the time the judgment is made. The evaluation of an object as 'pleasant' or 'unpleasant' tells nothing directly about the felt experience of the subject.

This was demonstrated experimentally by Yokoyama[6] for the method of paired comparisons. He concluded that "conscious P and U drop away from the affective perception in the course of its decay, thus following the law of perceptual contextual sensations," and that "*P* and *U* (*of the method of paired comparisons*) are most universally and definitely statable as meanings." These meanings of P and U should not be confused with affective experiences.[7]

In the above experiment 2 *S*s showed an appreciable degree of constancy in affective judgments to odors during a period of 12 experimental days. Possibly, however, the *judgments* are constant whereas the *felt experience* tends to drop out with habituation. The ambiguity of the affective judgment regarding felt experience makes it difficult to interpret the results.[8]

To make the case clear let us look at it physiologically. When an odor is presented for the first time to a subject it may bring

[5]See the writer's study referred to in foot-note 4.

After the above experiment a few of the odors were selected and given to a group of 57 additional *S*s. The aim of the test was to check the predictive value of the results. With the smaller group No. 30 was reported U by 2 *S*s, as I by 1, and as P by 16. We predicted that it would be reported P by the larger group; the result was U 7, I 0, P 50. With the smaller group No. 16 was reported U by 15 subjects, I by 2, and P by 2. We predicted that it would be reported U by the larger group; the result was U 55, I 0, P 2. Predictions regarding like and dislike are thus possible for a group; but this, of course, does not mean that we can tell how the individual *S* will react in any particular case.

[6]M. Yokoyama, The nature of the affective judgment in the method of paired comparisons, this JOURNAL, 32, 1921, 369.

[7]For discussion of this point see P. T. Young, The coexistence and localization of feelings, *Brit. J. Psychol.*, 15, 1925, 356 f.

[8]This JOURNAL, 40, 1928, 399-400.

about a smile, muscular relaxation, or a mild tendency to vomit or to hold the breath. Along with these bodily changes may be some verbal expression such as "the odor was P" or "I didn't like that." With repeated presentations of the odor the verbal reactions may

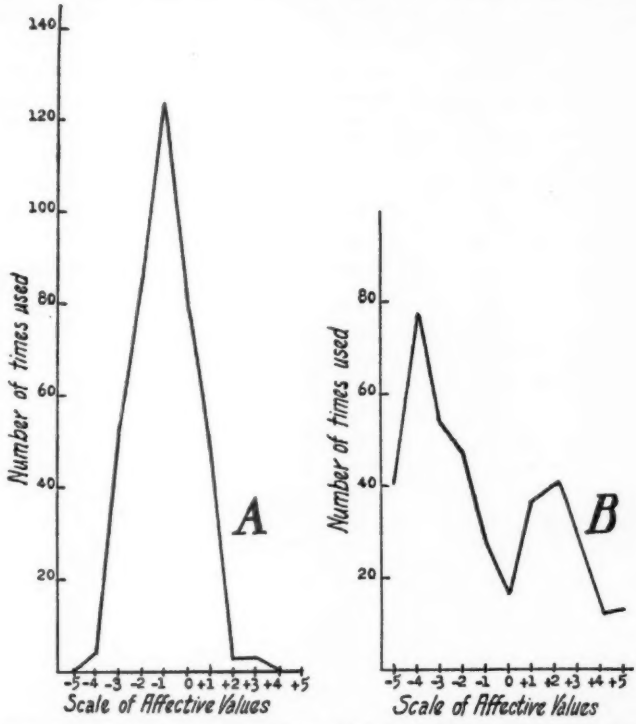

FIG. 1. SHOWING THE DISTRIBUTION OF *A*'S AND *B*'S AFFECTIVE JUDGMENTS

persist while the smiling, relaxing, vomiting, holding of the breath, etc. disappear. Thus the verbal equivalent may remain in the absence of the other reactions. In every-day life we know that this happens and that such a statement as "the object was pleasant" is equivocal regarding the emotional and affective reactions of the *S* at the time the statement is made.

This equivocal nature of the affective judgment is a difficulty common to all forms of the so-called method of impression; scale

of values, paired comparisons, order of merit, single stimulus, method of choice, etc. The methods are of little value in the study of felt experience.

(2) *Effect of S's attitude.* A second difficulty with the foregoing experiment is that every judgment depends to some extent upon the *S*'s attitude towards the scale of values. To illustrate the effect of attitude let us consider the above experiment in which two *S*s, *A* and *B*, made affective judgments upon the 33 stimulus-objects for 12 successive experimental days. With what frequency was each point on the scale of values used by these 2 *S*s for the total experiment? Fig. 1 answers the question graphically, showing the distribution of the 396 affective judgments given by each *S*.

For *A* the curve is unimodal and the extremes of the scale are rarely if ever used; the most frequently used category is -1. For *B*, on the other hand, the curve is bimodal with a distinct drop at 0. The modes are -4 and $+2$; the extremes of the scale are used with significant frequencies.

How shall we interpret the difference between the curves? If we were to assume that the curves show something about the affective processes, we should proceed as follows. Both curves show a predominance of U over P which indicates that, with the selection of stimuli in the experiment, U experiences occurred more frequently than P. *A* did not experience intense P and U during the experiment but found most odors indifferent or slightly U. *B* was more sensitive to the odors than *A*, *i.e.* more apt to experience either P or U and especially more apt to experience the relatively intense P and U.

Although such an interpretation in terms of affective process is possible, it is nevertheless equivocal. A second type of interpretation follows. The curves indicate that the *S*s apprehended the scale of values differently. *A* probably considered the scale to be a single continuous series whereas *B* looked upon it as two scales, one for P and one for U with indifference between them. The extremes "very great pleasure" and "very great displeasure" were interpreted differently by the *S*s. For *A* the interpretation of $+5$ and -5 was such that affective processes of these intensities did not occur in the experiment. For *B* the scale was apprehended in such a way that the extremes occurred with significant

frequencies. The difference between the curves is a matter of the *S*'s attitude—of the way in which the scale is apprehended. Since the use of the scale is an individual and purely subjective matter we have no way of knowing whether the intensity of P felt by *A* and designated +3 corresponds to the intensity felt by *B* and designated +3.

Thus there are at least two ways to interpret the curves. If we assume that the *S*s are alike in their affective processes, we may then conclude that the curves show a difference of attitude towards the scale of values. If, on the other hand, we assume that the *S*s are similar in their attitude towards the scale, we may conclude that they differ in their affective processes. The latter interpretation is rendered more uncertain by the possibility that an affective judgment may be made in the absence of affective experience.

Unfortunately we have no statement from the *S*s regarding their attitude towards the scale of values. We may appeal, however, to other experimental evidence. Conklin[9] in 1923 published a study of the scale-of-values method. He used a scale of 13 points:

1	Greatest possible pleasure	565
2	Very, very great pleasure	359
3	Very great pleasure	530
4	Great pleasure	1025
5	More than a little pleasure	633
6	Just the slightest pleasure	785
7	Neutral, neither	1196
8	Just the slightest displeasure	851
9	More than a little displeasure	779
10	Great displeasure	1232
11	Very great displeasure	581
12	Very, very great displeasure	479
13	Greatest possible displeasure	1179

The figures at the right show the total frequencies for every category of judgment by a group of 1699 public school pupils when judging propositions such as "to do ridiculous initiation stunts in public." One is impressed immediately by the high frequency for the values 4, 7, 10, 13. Regarding this matter Conklin writes: "It becomes evident upon examination of these distributions and graphs that there is a free use of terms 1, 4, 7, 10, and 13. The frequent use of these indicates that they describe clearly distinguishable varieties of experience." The writer would like to suggest that these high values do not indicate "clearly distinguishable varieties of experience" but rather a general preference for certain categories of judgment on the scale of values. The scale is symmetrical and the points 1, 4, 7, 10, 13 are easily regarded as round numbers and convenient points of orientation towards the scale.

[9]Reference in footnote 4.

Conklin used the same scale to study belief and disbelief in propositions presented to the same *Ss*—the term "belief" being substituted for "pleasure" and "disbelief" for "displeasure." The curves for belief and disbelief also show marked peaks at the points 1, 4, 7, 10, 13 similar in every respect to those on the curves for pleasure and displeasure. Are we then to assume that the "experiences" of belief and disbelief indicated by points 1, 4, 7, 10, 13 are of more frequent occurrence than those indicated by other points? If so, it is a curious fact that the frequency distribution for belief-disbelief "experience" corresponds so closely to that for pleasure-displeasure.

Conklin's result reminds one of Coover's[10] well-known study of mental habit in relation to judgment. Bringing together available evidence from a wide variety of fields Coover demonstrated a preference for round numbers. In the twelfth and thirteenth census reports, for example, the figures indicate that persons give their ages most frequently in multiples of 5 and 10. Also when students' grades are given on a percentile basis the figures indicate that the teachers have a distinct preference for multiples of 5 and 10. Coover found that judges in giving criminal sentence have a marked preference for round numbers when the sentence is in years, and for quarters of a year when the sentence is in months. Further, in the estimation of star magnitudes a preference for round numbers is manifest. For example, one bit of evidence gives 1239 stars of the 6.5 magnitude and only 159 of the 6.6. Similar numerical preferences were found in the estimation of meteorological conditions and in guessing.

It is not surprising, therefore, that certain categories of affective judgment on Conklin's scale of values should be used with much greater frequency than others. Further evidence of number-preference may be indicated by the following experiment.

A random list of 43 common foods[11] was prepared and the list read slowly to the experimental group of 19 *Ss*. The group was instructed to estimate their degree of liking or of disliking for the food suggested, in terms of the above 11-point scale which was printed on the blackboard. In explaining the scale the *Ss* were told to use +3 and −3 for average liking and disliking. Possibly this explanation had some effect upon the result.

Fig. 2 shows the frequency with which each point on the scale was used by the total group. About 80% of the judgments are on the P side of the scale.

[10] J. E. Coover, *Experiments in Psychical Research at Leland Stanford University*, 1917, Part III, Mental habit and inductive probability, 229 f.

[11] The foods suggested are recorded below. Qualifications in parenthesis were made at the time the test was given in answer to the questions of the group. The general instruction was to consider the food as well-cooked but plain, without sauce or dressing, and to estimate immediately the degree of like or dislike for the suggested food: fried parsnips, stewed onions, raw oysters, fried oysters, fresh milk (not warm), raw carrots, uncooked apple, orange, banana, pineapple (raw), vanilla ice cream, mashed potato, raw tomato, cooked tomato, crisp bacon, soft (not crisp) bacon, roast beef (rare), roast beef (well done), mutton chops, boiled cabbage, saur kraut (cooked), cauliflower, boiled turnip, raw young onions, radishes, fried chicken, stewed chicken, baked chicken, fried fish, baked fish, boiled egg (soft), fried egg, raw egg, lettuce, lemonade, buttermilk, coffee with cream and sugar, plain coffee (nothing added), tea with cream and sugar, plain tea, cocoa (sweetened), dry toast, buttered toast.

The significance of this fact is obvious. Common foods are generally liked; if disliked, they would not be in common use. The curve is bimodal with one mode at +3 and the other at −3 and the lowest point curiously enough at −1. The relatively high frequencies for +3 and −3 correspond to Conklin's result

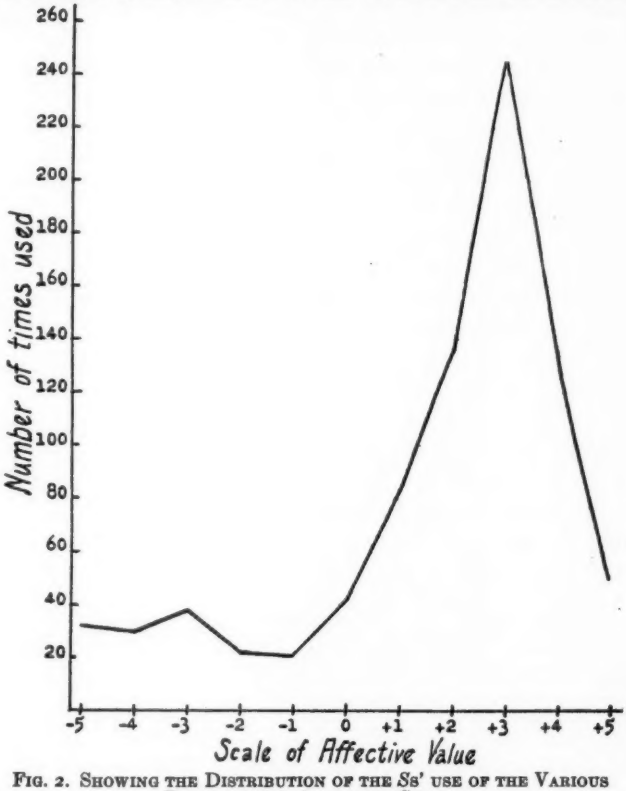

FIG. 2. SHOWING THE DISTRIBUTION OF THE *Ss'* USE OF THE VARIOUS POINTS OF THE AFFECTIVE SCALE

that the central points of the scale for P and for U have relatively the greatest frequency.

In view of the above evidence we conclude that the *S's* attitude towards the scale of values is one factor which determines the result. The way in which he apprehends the scale and his preference for certain categories of judgment are factors involved in this method.

(*C*) *Utility of the scale-of-values method.* (*1*) *Rating of objects.* A great deal of affective psychology has concerned itself

with the evaluation of objects in terms of P and U. Typical questions are "Which of these bits of colored paper is the most P?" "How may these odors, tones, drawings, geometrical figures, etc. be rated as regards P and U?" Similar questions have been formulated on the level of objects and in terms of objects.

The evaluation of objects may have some practical importance. A merchant may want to know in advance how this or that fabric will be liked by the public. A customer may face the problem of rating affectively a series of photographic proofs. In these and similar cases some practical method for determining relative like and dislike is needed.

But the psychologist questions whether such gross rating of objects has any scientific importance. Suppose all the objects in the world were rated affectively by all the persons in existence, what would be gained? This would throw little light upon the nature of felt experience, the nature of affective reactions in the organism, and other matters of psychological importance.

(*2*) *Study of affective reactions.* *C*, a woman, made successive affective judgments upon the 33 stimulus-objects under conditions identical with those described above for *A* and *B*. On the fourth day of the series *C* was distinctly in a U mood. She was dull, cross and her general behavior corresponded to the verbal report of mood. On this day U odors were judged more intensely U, P odors were less P, weak P became U, the empty bottle was judged as −3. In a word, the U mood accompanied a decided swing of all judgments towards U. This may be shown statistically by adding together all of the U-values and also all of the P-values for every day of the experiment.

Day No.	1	2	3	4	5	6	7	8	9	10	11	12
U	−68	−60	−54	−86	−67	−77	−67	−67	−58	−63	−69	−66
P	+24	+40	+33	+ 8	+25	+19	+27	+29	+20	+25	+33	+32

The writer hesitates to interpret the result but the fact is clear that a distinctly U-mood on day 4 coincided with a decided swing toward U affective judgments.

The above result shows that it may be possible to study daily variations in affective reaction and individual differences within a group. The main requirement is keeping the stimuli and objective conditions constant.

SUMMARY

The scale-of-values method may yield statistically uniform results. The interpretation of these uniformities is doubtful because (a) the affective judgment is equivocal regarding the existence of felt experience, and (b) the *S*'s attitude towards the scale is a factor determining judgment.

The method finds application in two ways; (a) in the affective evaluation of objects and (b) in the study of affective reactions of the *S*. The former may be of practical value but is of little psychological importance; the latter opens the way to the study of individual and group differences in affective reaction.

IX. THE POINT OF VIEW OF AFFECTIVE PSYCHOLOGY

The problem. A good way to approach affective psychology is by making an actual demonstration. Prepare (a) a bottle of dead fish, (b) several perfumes, (c) a lump of sugar, (d) an intense quinine solution, (e) a soft warm object, and (f) a needle for puncturing the skin. When these and similar objects are presented to the nose, tongue or skin, as the case may be, *S* will normally report that (a) (d) (f) are displeasing and (b) (c) (e) are pleasing. The demonstration presents most convincingly a central problem of affective psychology. Some situations evoke an experience described as pleasing, agreeable, acceptable; others an experience described as displeasing, disagreeable, unacceptable. The genuineness of the difference is guaranteed by the statistical constancy of the reports when the demonstration is given to a group.

The demonstration may be viewed in two ways, either from the point of view of the *S* or from the point of view of the demonstrator. From the first point of view there is a *felt experience*—a pleasing or displeasing character to the total experience—which is verbally reported. From the second point of view there are certain *affective reactions*—smiling, frowning, withdrawing, relaxing—which the demonstrator may observe. Ordinarily the affective reactions may be distinguished from others not markedly affective, such as counting out loud, opening and closing the eyes, moving the fingers, etc.

The writer believes that both aspects reveal a single natural process going on within the organism. Any one can observe

both aspects of the affective process by going through the demonstration first as *S* and then as demonstrator. The present paper is limited to the first of these aspects.

The nature of felt experience. Two views of the nature of felt experience are rejected. First, there is the view that P and U are attributes of sensation. This has been discredited by Külpe.[12] Secondly, there is the view that P and U are statable as meanings. This view was expressed by M. Yokoyama.[13]

Two views of the nature of felt experience demand serious consideration; the sensational and the traditional non-sensational views. These will constitute the main topic in the following discussion.[14]

(*A*) *The sensational view of felt experience.* (*1*) *The method of sensory observation.* An analysis of the method of observation in the natural sciences brings to light the following facts.[15] (1) Every observation involves some phenomenon which constitutes its object. Men of science have a bias towards visible phenomena, but this is not a necessary state of affairs since pains, pressures, tones, noises, tastes, smells, etc. are all observable as truly as visible objects.[16] (2) Every observation involves a set, or determination, on the part of the observer. This, as we have pointed out,[17] may be conceived in either of two ways: (a) as a physiological adjustment of the neuromuscular system determining bodily reaction, or (b) as a logical proposition stated in

[12]O. Külpe, *Outlines of Psychology*, (E. B. Titchener, tr.) 1893, 227 f.

[13]This JOURNAL, 32, 1921, 357-369. The meaning hypothesis is rejected on the following grounds: (a) The term 'meaning' is ambiguous and ill-defined. Psychologists would do well to follow Professor Calkins' advice and "to abjure entirely the use of the slippery conception, 'meaning' " (this JOURNAL, 39, 1927, 7-22). (b) Studies of the writer (see reference in footnote 7) have repeatedly pointed out the difference between purely cognitive and affective processes. It is important to distinguish between affective and purely cognitive processes rather than to blur the distinction by the use of such a term as 'meaning.'

[14]An historical survey will not be attempted. The reader would do well to see E. B. Titchener's *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, Chap. 3, in which C. Stumpf's view that affections are *Gefühlsempfindungen* is presented and discussed.

[15]Much of the material in the following discussion was presented at the Eighth International Congress of Psychology, Groningen, Holland, September, 1926, in a paper entitled, *An analysis of observation in the field of affective psychology.*

[16]H. L. Hollingworth, Sensuous determinants of psychological attitude, *Psychol. Rev.*, 35, 1928, 93-117.

[17]P. T. Young, The phenomena of organic set, *Psychol. Rev.*, 32, 1925, 477-478. Set is also implied in such words as 'purpose in observing,' 'attitude,' 'preparatory adjustment,' '*Aufgabe*,' '*Einstellung*.'

words or other symbols without reference to the bodily basis. (3) The bodily process of observing may be reduced to simpler bodily processes in the receptors, neurones, muscles, and other structures. This view of observing is incomplete because it leaves out of account the phenomenon observed. (4) Scientific observation is generally followed by a report which expresses in words or other symbols the phenomenon observed. Record is made of the object, event or relation which is observed.

Within psychology observation is often of the above type. For example, when Fernberger[18] observed the hallucinatory images after taking the drug *peyote* (*Anhalonium Lewinii*) his manner of observing was identical with that found in the other sciences. In the numerous investigations upon sensory processes one can find illustrations without limit of this objective type of sensory observation.[19]

(*2*) *Titchener's view of the affective processes.* Titchener has stated that the method of psychology is observation. "And observation," he writes, "implies three things: a certain attitude towards phenomena, a vivid experience of the particular phenomenon which is the object of observation, and an adequate report of this experience in words."[20]

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[18] S. W. Fernberger, Observations on taking peyote, this JOURNAL, 34, 1923, 267.

[19] Granting that the method of observation is the same inside and outside of psychology, the question arises "How does psychology differ from physiology, physics, chemistry, and the other natural sciences?" In the writer's view the traditional gulf between physical and psychological science exists only for those who have made it. The main difference between psychology and the physical sciences is the place where explanation is sought. The psychologist looks always to the body of the observing organism to explain phenomena. Ernst Mach summed up the matter very well when he wrote: "A color is a physical object so long as we consider its dependence upon its luminous source, upon other colors, upon heat, upon space, and so forth. Regarding, however, its dependence upon the retina . . ., it becomes a psychological object, a sensation. Not the subject, but the direction of our investigation, is different in the two domains." E. Mach, *Contributions to the Analysis of the Sensations*, (C. M. Williams, tr.) 1910, 14-15.

[20] The word 'introspection' has been avoided because it is frequently used as a reproach or condemnation of something bad in the methodology of psychology. Titchener is often regarded as the arch-introspectionist, but he himself expressed hesitation in the use of the word. I quote: "I have avoided the term 'consciousness.' Experimental psychology made a serious effort to give it a scientific meaning; but the attempt has failed; the word is too slippery, and so is better discarded. The term 'introspection' is, I have no doubt, travelling the same road; and I could easily have avoided it, too; but the time is, perhaps, not quite ripe." (E. B. Titchener, *A Beginner's Psychology*, 1922, x.) Titchener has also stated precisely what he means by introspection. Generally he means scientific observation within psychology in the sense that a discrimination between white and black is an introspection. E. B. Titchener, Prolegomena to a study of introspection, this JOURNAL, 23, 1912, 427-448; The schema of introspection, *ibid.*, 23, 1912, 485-508. See also H. P. Weld, *Psychology as Science*, 1927, 66.

Regarding the nature and place of observation in psychology see a recent paper by C. C. Pratt, Theoretical studies from the Harvard psychological laboratory; faculty psychology, *Psychol. Rev.*, 36, 1929, 147 f.

Now according to Titchener's earlier view of affective experience the chief mark of P and U was lack of clearness or vividness. "Attention to a sensation means always that the sensation becomes clear; attention to an affection is impossible. If it is attempted, the pleasantness or unpleasantness at once eludes us and disappears, and we find ourselves attending to some obtrusive sensation or idea that we had not the slightest desire to observe."[21] Elementary affective processes have the attributes of quality, intensity and duration but lack the attribute of clearness. When Titchener writes that affective processes lack clearness he means precisely what he says although this point has been frequently misunderstood.[22]

Titchener's position contained a fundamental contradiction. To say that P and U are not clear is to say that they are not observable (as observation is defined). How then can the affective processes be brought within a psychology based upon direct observation as its fundamental method? Titchener was aware of the difficulty and attempted to get around it in the following way: "In the case of sensation, the observer is set or disposed, beforehand, to attend to sensation and to report upon sensation; the sensation comes, and is attended to; and the report which follows is determined, under the influence of the preliminary set or disposition, by the nature of the sensation. In the case of feeling, the observer is set to attend to sensation, but to report upon the feeling which accompanies the sensation; the sensation comes and is attended to; and the report then describes, under the influence of the preliminary set, the feeling which accompanied the sensation. That sounds a little paradoxical; but the method is not difficult in practice; and it has the advantage that we can use all manner of sensory stimuli (colours, tones, everything) in our study of feeling."[23]

Despite the attempt to cover up a difficulty Titchener's logic is bad and reminds one of the attempt to eat the cake and keep it too. He admits into his system of psychology non-clear, non-observable affective processes. Hence in the interests of consistency he would do well to change the statement regarding psychological method so as to include processes which are reportable but not attentively observable, namely the affective processes.

Doubtless Titchener's ground for retaining non-sensory elements in his system of psychology rested upon the belief that pleasantness and unpleasantness are similar in many respects to sensory processes and are, perhaps, unde-

[21]E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 69.

[22]Titchener's position is clearly shown in the following reply to Watt: "Meanwhile, Dr. Watt's own logic has not been adequate to the theory which he attacks. 'Are our feelings so unclear?' he asks. As well might he ask: Are our spaceless sensations so very small? A process can be unclear only if it can also, under other conditions, be clear; and, on my view, the affective processes are neither clear nor unclear, but non-clear; they do not vary at all, as sensations vary, from vivid to dim, from clear to obscure; they show, in experience, neither vividness not obscurity; they lack the attribute of clearness in precisely the same way in which they lack the attribute of space. . . . And the phrase 'total absence of clearness' means for me what it says: which is something very different from 'total obscurity.'" E. B. Titchener, Feeling and thought: a reply, *Mind*, N.S. 20, 1911, 258-260.

[23]E. B. Titchener, *A Beginner's Psychology*, 1915, 80.

veloped sensations. P and U, Titchener states, resemble organic sensation; they have the attributes of quality, intensity, duration; further, they show adaptation, and possibly in other ways are similar to sensation. Titchener is definitely biased in favor of a sensational view of P and U. He favors the view that P and U have peripheral organs. I quote: "The writer hazards the guess that the peripheral organs of affection are the free afferent nerve-endings —what are ordinarily called the free sensory nerve-endings—distributed through the various tissues of the body; and he takes these free endings to represent a lower level of organic development than the specialised receptive organs, or organs of sense. Had mental development been carried further, pleasantness and unpleasantness might have become sensations: in all likelihood would have been differentiated, each of them, into a number of sensory qualities."[24]

We now know that Titchener's sensationalistic leanings finally dominated his theorizing and that toward the close of his life he came to regard P and U as sensory processes. The earlier view of affective processes gave way to a thorough-going and consistent sensationalism. Titchener's belief that affective processes are similar to sensations in several fundamental respects foreshadows a totally different systematic position which in recent years has come out of the Cornell Laboratory.

The modern view finds its clearest expression in a paper by J. P. Nafe[25] and may be described as follows. According to Nafe, Titchener accepted the following list of simple touch qualities: tickle, itch, contact, pressure, prick, clear pain, quick pain, strain, drag, ache, cold, warmth, heat, bright-pressure, dull-pressure. The last two are important in the present connection so we quote from Nafe in detail.

"*Bright-pressure.* Bright-pressure and dull-pressure are the two of Titchener's qualities least known. He describes bright-pressure as a diffuse bodily feel, bright, buoyant; an experience that goes with a general feeling of well being. It is not described as containing an element of pressure, pressure being, in this instance, a general term. The experience could as well have been called a 'felt-brightness' and it does not necessarily occur so diffused as to include the whole body.

"*Dull-pressure.* Dull-pressure is a diffuse bodily feel also, but duller, heavier, more as one feels the day before one comes down with influenza, yet not strong like drag. These terms came into use with a fairly large group but have never been generally accepted."

According to Nafe[26] felt pleasantness when directly observed *is* bright-pressure and unpleasantness *is* dull-pressure.

Writing upon the change in Titchener's formulation Nafe states[27] that the criterion of affective experience had been its unobservability; but by 1921 Titchener "had realized the impossibility of such a condition of affairs without much urge from the outside world." Consequently an experimental problem

[24]E. B. Titchener, *A Text-book of Psychology*, 1909, 261.
[25]J. P. Nafe, The psychology of felt experience, this JOURNAL, 39, 1927, 367-389.
[26]An experimental study of the affective qualities, *ibid.*, 35, 1924, 507-544.
[27]*Op. cit.*, *ibid.*, 39, 1927, 386.

was formulated and this gave rise to the view expressed by Nafe. "This problem restored the affective qualities as observable phenomena and furthermore put them in the same category with other experience (sensory); but they were found to occur only as undivided parts of other experiences, never alone." Nafe concludes "that affective and emotional experiences are fundamentally similar to other ('sensory') felt experiences." The same view has been expressed by Hoisington.[28]

Thus if we look at the case historically it is obvious that Titchener's systematic position has changed. In 1918, when the writer was experimenting in his laboratory, P and U were regarded as similar in several respects to sensation but different in that they lacked the attribute of clearness. Affective processes were unobservable (as the method of observation was defined) and bright-pressure and dull-pressure were not yet in the air. Ten years later P and U are described as observable processes—P a bright-pressure and U a dull-pressure. The original inconsistency in Titchener's systematic formulation has been resolved by the 'discovery' that P and U are observable sensory processes. Titchener's latest position is a clean-cut sensationalism which is logically self-consistent. The earlier affective processes have vanished!

The change has been in the direction of logical consistency and we may now summarize the present position as follows: The method of science is direct observation.[29] This method is to be held consistently. Direct observation gives only observable processes. They alone constitute the subject matter of a scientific psychology. Non-clear affective processes do not belong within psychology.

Criticism of the sensational view of affective processes. It is an experimental problem to determine precisely what is observed when a sensational attitude[30] is taken towards P and U. If P and U are observable, they are either (a) ultimate elementary processes in the cutaneous-kinesthetic-organic group, or (b) characteristic integrations which may be reduced through analytical observation to elementary components.

The writer is skeptical about the bright- and dull-pressure view for reasons given earlier in this series of studies. There is doubtless an association between P and bright-pressure and between U and dull-pressure; but the existence of an indispensable sensational condition of felt experience has yet to be demonstrated. The statement that P *is* a "bright-pressure" implies that every

[28]L. B. Hoisington, Pleasantness and unpleasantness as modes of bodily experience, *Wittenberg Symposium on Feelings and Emotions*, 1928, 236-246.

[29]See footnote 20.

[30]P. T. Young, Studies in affective psychology, this JOURNAL, 38, 1927, 170-171. In the present connection it may be well to note the view of Lehmann that "Die historische Tatsache aber, dass die Psychologen über die Natur der Gefühle nicht einig werden können, beweist zur Genüge, dass Lust und Unlust keine Empfindungen sind; sonst hätte man sich längst verständigt." A. Lehmann, *Die Hauptgesetze des menschlichen Gefühlslebens*, 1914, 21.

time P is felt sensorial observation will yield a "bright-pressure" and never a "dull-pressure" and never the absence of all pressure experience. The matter needs further investigation in which negative cases are stressed.

Crosland[31] working upon the problem of forgetting studied affection incidentally and found no constant relation between occurrence of affective process and occurrence of any particular kinaesthetic and organic process. In an earlier study of this problem I concluded that there is no sensory *sine qua non* of affection.[32] The situation seems to be the same as that with the so-called expressive methods. There are associations—as between P and vasodilation or U and vasoconstriction—without any one-to-one relationships. There are no hard and fast connections.

This being the case we must produce further experimental evidence before concluding that P and U are directly observable as observation has been defined. At present I do not see how one can claim that P *is* this or that process or integration.

Let us now look at the situation from the logical point of view. We started by defining the method of observation, determined to hold to that method consistently and to accept whatever result it might yield. Prior to experimentation we may know on purely logical grounds that the method of observation is limited to the study of observable processes. From the nature of the case non-observable processes do not and cannot come into the picture. This is a truism.

If we lay it down as a matter of definition that whatever is psychologically observable is a sensory process, we have solved the problem in advance. Whatever the method yields is sensory. The method cannot give any evidence for the existence of non-sensory, non-observable processes.

Thus from the logical point of view Titchener's sensationalism does not nor can it settle the question regarding the existence of non-sensational processes of experience. Sensationalism is merely a determination to hold to a particular point of view and method and to accept whatever results. In itself this is commendable for scientific advance is achieved through specialization.

[31]H. R. Crosland, A qualitative analysis of the process of forgetting, *Psychol. Monog.*, 29, 1921, (no. 130), 143. Crosland also notes (92 f.) that there may be all possible relations between strain and relaxation, on the one hand, and P and U, on the other hand; but in general U most frequently occurred with kinaesthesis or strain and P with decrease of strain or relaxation. This general conclusion agrees with my own on the point (this JOURNAL, 32, 1921, 39 f.). The two studies were done independently and published the same year.

[32]*Op. cit., ibid.*, 32, 1921, 39 f.

(*B*) *The non-sensational view of felt experience.* It was previously stated that phrases such as "I like it," or "I dislike it" may indicate the existence of felt experience without being sensational descriptions of it.[33] A verbal report may inform us of the qualitative character, the intensity, or the temporal course of affective experience. It may be argued, of course, that such phrases do not *describe*[34] the experience but merely indicate a gross pattern which, if fully *described*, would reduce to sensational processes.[35]

Whether the functional report is descriptive or not depends entirely upon what we mean by description. The functional report is not analytical; nor does it presuppose the sensational point of view. It does, however, give significant information about human experience; and it makes possible the study of significant problems which lie entirely outside of sensational psychology.

These problems lie in several fields, in differential psychology (see Part VIII), in physiological psychology (see Part VII), and in social and abnormal psychology.

If we admit that the functional report is a valid psychological method distinct from the method of sensory observation, we must not forget that there are still dangers and limitations. One of the chief difficulties with the functional report is the ambiguity involved. We have repeatedly pointed out that a report may confuse the merely cognitive evaluation of an object in terms of P and U with the direct report of felt experience. This ambiguity is responsible for much of the disorder prevalent in affective psychology to-day.

Let us now consider the question whether there exist any non-sensational affective processes of experience. Clearly if we start from the bias of sensationalism, the question can be answered in only one way. The determination to limit the data of psychology to those gained by the method of direct sensory ob-

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[33] This JOURNAL, 38, 1927, 172.

[34] See Hoisington, *op. cit.*, 236. See also R. H. Wheeler, Analyzed versus unanalyzed experience, *Psychol. Rev.*, 29, 1922, 425-446.

[35] Relative to this proposition it might be said that sensational psychology itself accepts the functional type of report as indicative of unanalyzed experience and at least to this extent recognizes the usefulness of the method. Further, it might be argued that the functional report is a kind of observation and description which, however, is not sensational.

servation solves the problem prior to experiment. The whole of human experience is sensory, if we are determined to view it that way. If, however, we admit the validity of the functional approach, the question appears in a new light. We may claim, if we will, that the functional approach yields a direct report of non-sensory processes and characteristics of experience, and that sensational analysis is beside the point. Or we may claim, on the other hand, that the functional method yields only reports of gross, unanalyzed experience. Practically it doesn't much matter which view of the case we accept. Experimental work from the functional point of view need not await a final answer to these systematic problems. Perhaps there is no final answer.

In general, the writer prefers to make no advance assumptions about experience but to start affective psychology with the concrete demonstration of genuine problems. Various methods, points of view and approaches may be distinguished. Sensationalism is not the whole story. The functional approach is also useful.

X. Some General Conclusions

In 1917-18 the writer experimented upon affective processes in the Cornell Laboratory and tacitly accepted the Titchenerian view that P and U are non-sensory processes of experience. Since that time further experimentation has led to considerable skepticism regarding the methods and significance of the earlier work. After several seemingly futile experiments the present series of studies was commenced in the hope that light would be thrown upon the methodological difficulties.

Looking back over the studies one principle stands out prominently, the relativism of facts of observation to methods and postulates. Two observers—a behaviorist and a Titchenerian psychologist—were placed in the same experimental situation (Part III) and each reported consistently with his own theoretical biases. Again, two groups of Titchenerians—one in 1918 and another ten years later—in the same laboratory gave wholly different results. What follows? The only fair conclusion is that the facts of affective psychology are largely *made* before experimentation is commenced, *made* by the training, education, theoretical views of the subjects. The faith of the physical scientist that there are facts of observation independent of the observer is not valid. It is shattered by the finding that to a great

extent facts of observation may be made to order. One can train a group of observers to report A = B and another group to report A ≠ B.

The situation needs to be viewed as a problem in the psychology of opinion. In the everyday world men differ in political, ethical, religious, social, philosophical, scientific and other opinions. Here and there two individuals or a group may agree but a universal agreement is out of the question. In psychology the case is the same. The attitudes, points of view, theoretical beliefs and hypotheses of psychologists have *made* affective psychology what it is to-day. Even the 'facts' from which we start have to a large extent been *made* in advance.

The two roads. We come now to the parting of the ways. (1) We can hold dogmatically and persistently to some particular point of view, method, system of postulates, and accept the facts which our bias yields, rejecting all else as valueless, or (2) we can attempt to define psychology in such a way that all biases, attitudes, points of view are on the same level as parts of nature. In the latter case we admit freely that facts of observation are completely relative to biases of the observer.

Along the first road we have the satisfaction of distinguishing between true and false in terms of distinctions which have been made in part prior to experimentation. Along the second road the futility of attempting to discover 'truth' by present methods is granted; facts of observation are completely relative to a postulational system. The first road defines sharply the method of sensory observation and holds to it through thick and thin. This determination yields a sensationalism in which uniformities are discovered, facts ordered, and principles formulated. The second road reveals at the start that the method of observation has limitations when used as an instrument in search for 'truth.' The attempt to determine what is 'true' and what is 'false' along present lines is abandoned.[36]

The recognition of the principle that facts of observation are completely relative to the past history of the observer brings to light a fundamental principle of nature. Discrepancies within psychology are viewed exactly as we view discrepancies in the larger world where opinions clash. Psychologists after all are

[36] Cf. *op. cit.*, this JOURNAL, 38, 1927, 188 f.

human beings. From a relativistic point of view one can view calmly the prevailing situation in contemporary affective psychology.

Available methods. If one desires to free psychology so far as possible from the opinion of psychologists and to rest the science upon the ultimates of human experience, the question of methodology arises. Apart from the above remarks the present series of studies has thrown some light on the methodology of affective psychology. A brief summary may be in order.

Part VI demonstrated that problems of relative preference can be studied with animal subjects.[37] Employing this method one could easily work for a life-time upon problems of like and dislike. The method is behavioristic and physiological. It has the advantages of physical science, but it tells nothing directly about felt experience.

Part IX presented the affective problem in human psychology by means of a demonstration.[38] If we accept the functional type of report, various lines of investigation in differential, physiological, social, abnormal, and applied psychology are in plain view before us.

As regards sensationalism the future is good. The sensational approach to problems of feeling and emotion presupposes certain postulates and methods. It presupposes that the observers have an understanding of the psychological vocabulary of the day, that they have had a specialized type of training. The subjects are for the most part trained psychologists. There is no doubt that the careful extension of sensational psychology to the problems of feeling and emotion will bring to light matters of considerable interest and theoretical importance.

Our study has not carried the affective problems into the fields of the abnormal and the social in psychology. In these fields the functional approach is best, for the patients and subjects are not trained psychologists. Ordinarily in the fields of the abnormal and of social psychology no one troubles about the truth or falsity of some particular attitude. It is common to start from the verbal and other expressions of the subject and to account for them genetically. The psychological competence of the subject does not come much into the picture; but the competence of the investigator is a matter of great importance.

[37] *Op. cit., ibid.*, 40, 1928, 372-394.
[38] *Supra*, 27.

ON MAYER'S "RESIDUAL SONOROUS SENSATION"

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In 1874 Mayer published an account of a short series of experiments in which he attempted to measure the duration of what we would today call the positive after-image of tone.[1] In these experiments he served both as experimenter and observer. The following year he repeated the experiments using two observers whom he considered to be more competent than himself for this work because they were musicians.[2] He waited, so he says, for 19 years in the hope that other experimenters would repeat his work. Finally, in 1894 he ceased waiting and extended his researches himself.[3] In this latter series the extension was in two directions: he introduced variations of his original method and he employed more tones in his series of stimuli. In all the experiments of the three periods he relied upon the same fundamental devices. The first was a source of tone which was conducted to a specially constructed disk resembling a siren. This disk served as interrupter of the tone. The second was another conduction tube which picked up the interrupted tone and delivered it to the ear of the observer. The minor variations of this fundamental technique did not materially alter the results. For this reason it is not necessary here to do more than indicate the general method followed by Mayer.

For sources of tone he used tuning-forks with resonators. The resonator was mounted beside the fork with the nipple of the resonator extending laterally toward the siren disk. The siren was brought very close to the resonator, thus imprisoning the tone as much as possible within the conduction system. On the side of the siren opposite to the mouth of the resonator the conduction tube was led away to the ear of the *O* from a point as near to the siren as possible. When the hole in the siren lay in the path of the tone, conduction was unobstructed and the tone arrived at the ear of the *O* at full intensity. When the solid wood of the siren, a moment later, moved into the path, the tone was

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[1] A. M. Mayer, The determination of the law connecting the pitch of a sound with the duration of its residual sensation, *Amer. J. Sci. & Arts*, 8, 1874, 241-255.

[2] Mayer, A redetermination of the constants of the law connecting the pitch of a sound with the duration of its residual sensation, *ibid.*, 9, 1875, 267-269.

[3] Mayer, The law connecting the pitch of a sound with the duration of its residual sensation, *ibid.*, 47, 1894, 1-28.

cut off greatly. Mayer speaks of the obstruction as if it were entirely impenetrable—except in one passage in which he speaks with more exactness.[4] Here he remarks that the tone was not completely blocked but was only greatly weakened in intensity. In the individual experiments the rate of rotation of the siren was gradually increased from the initial rate, at which the tone clearly fluttered at the separate interruptions, to the critical rate at which the tone had just lost its last trace of flutter and had become smooth and continuous. At this stage the "residual" experience, as Mayer called it, was just long enough in duration to fill in the very brief interval of approximate silence between stimulations without loss of sensible intensity. From the determined rate of rotation and the known space between holes in the siren, he calculated the actual duration of the auditory after-image. His interest in the physics of the phenomenon led him to seek a formula by means of which the duration of the after-image for any tone might be calculated. This he achieved, with doubtful validity, under the conditions of his experiments.

Repetition of Mayer's Experiments

Procedure. In an effort to interpret Mayer's results in the light of our own experiments in which we found no auditory after-image,[5] we repeated his experiments both as he did them and with refinements of his technique. We duplicated his siren which was made of wood and cardboard fastened together in a two-ply disk. The holes were 1 cm. in diameter and the interspaces between holes 2 cm. The diameter of the nipple of the resonator was 0.5 cm. and the diameter of the conduction tube was 1 cm.—dimensions stipulated by Mayer. Unlike him we had only one siren which ran at different rates as a substitute for a set of sirens with varying numbers of holes. He rotated his siren by hand by means of pulleys and a belt. In all experiments the rotation of the hand was at a constant and identical rate. The rate of interruption was determined by varying either the number of holes in the siren or the diameter of the pulleys. We rotated our siren by an Edison electric phonograph motor whose speed could be increased or decreased at will and whose rate at any speed of rotation was presumably no less constant than Mayer's manual rotation. We succeeded in bringing the nipple of the resonator and the end of the conduction tube very close to their respective faces of the siren but we were unable to equal Mayer's precision. Our disk was very slightly warped and the consequence of this was that during rotation it varied from a position of just not touching the tube or resonator to a maximum separation from either of approximately 0.5 mm. Mayer's more perfect siren, he says, was kept at 0.3 mm. from the resonator. Upon investigation we found that the tone was not perceptibly louder on account of the additional orifice of 0.2 mm. Indeed, at full intensity, the sound penetrated the siren disk itself when both nipple and conduction tube were held tightly against it. For this reason we feel that the slight difference in precision between the two experimental arrangements is negligible. Mayer and his *Os* were in the same room with the instruments. On account of the escape of tone from the conduction system, we felt that it

[4] *Op. cit.*, 47, 1894, 11.

[5] H. G. Bishop, An experimental investigation of the positive after-image in audition, this JOURNAL, 32, 1921, 305-325.

was necessary to correct this inaccuracy in technique which Mayer disregarded. This was achieved by passing the conduction tube through a heavy stone wall so that the *O* was in one room and the apparatus in another. On account of their convenience of operation, we substituted Stern variators for forks. In other respects than those noted our conditions were like those of Mayer.

We referred above to the inexactness of Mayer's statement about the degree of intensity of the auditory experience which was perceptible during the moment when the siren obstructed the sound. Since he referred to this experience as if it were one of silence while in reality it was one of diminished tone, it seemed necessary to correct this additional error in technique. We, accordingly, reduced the intensity of the tone to the point at which there was complete silence when the siren blocked the conduction tube. Under these conditions, the tone was never strong but it was clearly audible when the hole was before the resonator with the siren at rest.

At slow speeds of rotation the character of the intermittent tone was similar to the interference phenomenon of beats. With increasing rate of interruption and with correspondingly increasing rate of beating, there arose an undertone of noise which finally blotted out the beating completely. So long, however, as any trace of the tone could be heard it was still beating. Long before we attained the number of interruptions which Mayer found to be necessary to give a smooth tone, this accessory noise obliterated the tone. Such results, failing to verify Mayer's claim that the tone became continuous, force one to the conclusion that his experiments emphasize not the alternation of sound and silence but rather the combined effect of the auditory experience of the moment consisting of the interrupted sound which came through the siren, the diminished intensity of sound which came directly to the *O*s from the instruments, and any additional sounds generated by the siren itself. One must conclude, furthermore, that Mayer did not measure the length of tonal after-image as he had intended but made his judgments upon the basis of a complicated auditory situation. He was not unaware of this as the following taken from the article of 1875 shows, "I will now describe the character of the successive sensations experienced when starting from rest we gradually increase the velocity of rotation . . . until the separate beats blend into a smooth, continuous sensation. When the disk is stationary, with one of the openings opposite the mouth of the resonator, . . . the ear will experience a simple sonorous sensation."[6] At the slower rotation, he goes on to say, two additional tones appear, one slightly above and one slightly below the beating tone. At increasing rates of rotation these tones diverge finally reaching a separation of the major sixth. "At the same moment, a resultant sound appears, formed by the union of the sound of the fork with the upper and lower of the secondary tones. This resultant is the second octave below the note given by the fork. On further increasing the velocity of the disk, the two secondary sounds and the resultant disappear, and the ear has alone the sensation of the simple sound produced by the beats of the fork which, at this stage of the experiment, blend into a smooth continuous sensation."

[6] *Op. cit.*, 268 f.

It was obvious, in the face of the conflict between what we found with the *Os* and the instruments in separate rooms and such a report as this, that we must repeat Mayer's experiments as he did them with instruments and *Os* in the same room, if we would duplicate his results. We, accordingly, arranged the apparatus to suit his conditions, continuing to use the variator as a source of tone.[7] As an aid to the *Os* (Misses Doris Dewey and Florence Manson, seniors, with psychological and musical training, and the writer), we placed an harmonium at hand by reference to which an *O* could check the pitch of any component tone which he detected at any time in the complex of sound. We began the experiment with the siren rotating at a somewhat slower rate and ended it with a somewhat more rapid rate of rotation than was stipulated by Mayer. During the stimulation the *Os* listened to the full complex as well as to the character of the stimulus-tone itself.

Results. We were able to verify the findings of Mayer to some extent. With him, we found that the tonal experience was beating in character and that it was complex. The primary tone lost its roughness and, at about the rate of interruption reported by Mayer, was smooth and continuous in character. We did not agree with him, however, that it was the only tone present at the incidence of smoothness. We could hear secondary tones in the complex when the primary had become smooth. In order to deal more fully with this disagreement it will be necessary to examine the physical and musical aspects of the whole auditory experience from the moment the siren begins to revolve until the end of the observation when the primary has become smooth.

The physical aspect is stated explicitly by Rayleigh.[8] He shows that we may expect to find, as components of the complex, the primary tone itself and any or all of four secondary tones which he designates as follows: if n represents the frequency of vibration of the primary tone and m the rate of interruption of the primary tone, then the complex will be composed, under the right conditions, of the primary, n, and the four secondary tones $n+m$, $n+2m$, $n-m$, and $n-2m$. As the rate of interruption increases, it is obvious that the primary tone will remain constant in pitch and that two of the secondaries will rise and two will fall in pitch. The point at which this progressive change will be broken off during the experiment will depend upon the auditory and musical character of the whole complex which has been attained at the moment the *O* decides that the primary is no longer interrupted.

[7]These experiments were conducted in the Psychological Laboratory of Cornell University.

[8]Lord Rayleigh, Acoustical observations, *Phil. Mag.* 9, 1880, 278-283.

If Mayer is correct, the *O* will judge the proper moment by the sole criterion of smoothness of the primary. If we are correct, smoothness will occur because of the musical character of the whole auditory experience at the critical moment. In other words, the judgment of smoothness is a matter of ultimate consonance, an ultimate consonance which was preceded by an earlier dissonance, during the single observation. A fuller discussion will make this cryptic proposition more clear.

During a single observation the auditory experience will advance from the initial, smooth note, *n*, to tonal complexes marked at first by very slow beats but subsequently by beats which increase in rate as the pitch of the secondary tones changes. Stated musically, the change will pass from a single tone at first through the progressively widening intervals of subminor second, minor second, major second to approximately harmonic or fully harmonic intervals, unless the observation happens to be broken off along the way. In the nature of the case the observation would not be broken off while the primary tone was heard to beat. If all four of the secondary tones were to materialize in the complex, one would have a variegated and constantly changing tonal experience, while the experiment was in progress. Quite obviously the musical unitariness of the whole would follow the laws of tonal fusion, consonance, and dissonance. Those combinations of tone which were relatively free from beats and which were, therefore, harmonious would be those combinations at which one would naturally leave off the observation because the beating had, for the first time, ceased. The hitherto kaleidoscopic change in auditory experience would, at the moment, have the appearance of coming to rest. Although the beating due to the interruptions and the well known beats of interference are unlike physically, they are similar as auditory experiences. We know that under the proper conditions the beats will attach to one or the other or both of the generators and, in such a case, even when produced by uninterrupted and continuously sounding generators, the generators will, one or both, be heard to beat. Since the beats will change in rate as the pitch of the continuously rising upper secondaries and the continuously falling lower secondaries changes, they will eventually become so rapid that they will fail to mask the secondary tones. When this occurs, the changing pitch of the secondaries will make them easy to detect, while the diminishing beats will be

relatively less conspicuous. At the proper moment, which will soon arrive, the full consonance or the approximately full consonance within the whole auditory complex will be judged as a smooth primary, in Mayer's type of observation, and in our type will be perceived as a smooth primary plus whatever consonant secondaries could be detected. The truly composite clang of a sounding string which is regularly perceived as a tonal unity supports this interpretation of the observation of the sounding complex. In these experiments in which the primary is reënforced to a considerable extent by transmission through the siren at all times, it is only natural that it should seem even more unitary than a clang. Since these observations must, we believe, be interpreted musically, we shall now examine them as musical experiences.

TABLE I

PRIMARY TONES AND CALCULATED SECONDARY TONES

m	n	$n-2m$	$n-m$	$n+m$	$n+2m$
36	128 c	56(64)	92	164(160)	200(192)
62	256 c′	132	194(192)	318(320)	380(384)
73	320 e′	174	247(256)	393(384)	466
88	384 g′	210	296	472(480)	560(576)
108	512 c″	296	404(409)	620(640)	728
126	640 e″	388	514(512)	766(768)	892
143	768 g″	482	625(614)	911(910)	1054
170	1024 c‴	684(682)	854(853)	1194	1364

TABLE II

MUSICAL NOTES HEARD IN THE TONAL COMPLEX

c	C c e g
c′	g c′ (e′flat, 307-e′) g′
e′	c′ e′ g′
g′	g′ (b′flat, 455-b′) d″
c″	(a′ flat- a′, 426) c″ (e″ flat, 614-e″)
e″	c″ e″ g″
g″	(e″ flat-e″, 640) g″ b″ flat
c‴	f″ a″ c‴

In Table I will be found the primary tones with their musical designation and their four secondaries calculated by the formulae given above. We have placed numbers in parentheses beside the calculated values of certain secondaries. These are the frequencies of vibration of the notes actually heard in the complex and identi-

fied by reference to the harmonium. The general agreement between calculated value and corresponding musical note is as close as might be expected.

Table II contains the musical names for the frequency values of all the tones which we heard in the complex as they are given in Table I. The several notes placed in parentheses are border cases and were judged as either the one or the other, but never both. The number in the parentheses is the frequency of the note omitted in Table I.

Examination of these results shows that never were all the secondaries detected. We discovered, in this connection, what Mayer did not report, namely, that the intensity of the secondary tone was so faint sometimes that it was lost at any but the most favorable position of the conduction tube in the ears. If we had searched for them all we might, perhaps, have found them. That, however, was not our problem.

In our interpretation of these results, it is their significance as musical intervals which is important. They cannot be unequivocally offered as measures of "residual" auditory sensation. In order, therefore, to render them more significant as musical phenomena we have assumed certain informative values of m and have computed secondary tones which theoretically might have manifested themselves in the complex. If m is assumed to be $n/20$, m will then be sufficiently small to produce values of the secondaries which will be rather near to one another and also to n itself. This assumption is significant because these values, when stated as musical intervals, should be a set of dissonant intervals. We find such to be the case. Expressing the intervals as decimals based upon any convenient value of n such as 20, we have the values of Table III. The first column shows the secondaries concerned. The second column shows the quotient derived from the ratio of the secondaries in the first column. The third column gives the name of the musical interval to which the interval of the secondaries in the same line of the first column most nearly corresponds. The fourth column gives the exact value of the interval named in the same line of the third column. For example, if $n=20$ and $m=n/20$, then $m=1$ and $n+2m=22$. It follows that $n:n+2m=1:22/20$ which is 1.1. In other words, if n has the value of 1, n+2m will have the value of 1.1. The excellent agreement between the second and fourth columns shows how nearly

the values of m and n, as assumed, and calculated according to the combinations of the first column of the table yield standard musical intervals.

TABLE III

RATIOS SHOWING DISSONANT TONAL COMPLEX

n+m:n+2m	1.047+	Minor Second	1.066+
n:n+2m	1.100	Minor Second	1.066+
n−m:n+2m	1.157+	Acute Major Second	1.125
n−2m:n+2m	1.222+	Minor Third	1.200
n:n+m	1.050	Minor Second	1.066+
n−m:n+m	1.105+	Grave Major Second	1.111+
n−2m:n+m	1.166+	Augmented Second	1.171+
n−m:n	1.052+	Minor Second	1.066+
n−2m:n	1.111+	Grave Major Second	1.111+
n−2m:n−m	1.055+	Minor Second	1.066+

Table III demonstrates that relatively small values of m, *i.e.* a low rate of interruption must inevitably be discordant. The only consonant interval shown is the minor Third. In the nature of the case, however, it may not occur alone since the primary will always be present. This being true, no possible combination of notes at $m=n/20$ can be consonant. Acording to our argument, therefore, Mayer's observations could not end here. Greater rapidity of interruption than $m=n/20$ must be resorted to if one is to arrive at consonance.

We were guided in our choice of intervals, which should either be fully consonant or more consonant than those in Table III, by Mayer's results as they are stated in Table IV. In the first line are the values of n, in the second line the values of m, and in the third, the values of m/n expressed reciprocally. Following the

TABLE IV

MAYER'S RESULTS EXPRESSED RECIPROCALLY

128	256	320	384	512	640	768	1024
36	62	73	88	108	126	143	170
1/3.5	1/4.1	1/4.4	1/4.3	1/4.7	1/5.0	1/5.3	1/6.0

implicit suggestion of these values in Table IV, we calculated other tables, below, using successively, $m=n/10$, $n/6$, $n/5$, and $n/4$. The values of $n/6$, $n/5$, and $n/4$ are suggested directly by Table IV. We chose $n/10$ because it is intermediate between $n/20$ which is too low and $n/6$ which is too high for the threshold of consonance. At $n/10$ we hoped to find either barely consonant

intervals or a few consonant and a majority of dissonant intervals, as a demonstration of the threshold of consonance at which, or just beyond which, the observation in a single experiment might end. Inspection of Table V, which shows the array of intervals at this value of m, reveals the fact that we are here close to or at the threshold we sought. Except for the Fifth which is perfectly consonant (but it could not exist alone) and the minor Second which is fully dissonant, the intervals are about as much consonant as dissonant. Mayer could, we believe, not quite break off an observation at $n/10$. Translating Table V into intervals in the key of C natural we would have *A*-flat, *B* (modified), *C*, *D* (modified), *E*-flat. The observation, obviously, could not end if all the notes were heard but it could end if the *B* and *D* were absent.

TABLE V

APPROXIMATE CONSONANCE-DISSONANCE THRESHOLD

n+m:n+2m	1.090+	Grave Major Second	1.111+
n:n+2m	1.200	Minor Third	1.200
n−m:n+2m	1.333+	Fourth	1.333+
n−2m:n+2m	1.500	Fifth	1.500
n:n+m	1.099+	Grave Major Second	1.111+
n−m:n+m	1.222+	Minor Third	1.200
n−2m:n+m	1.375	Fourth	1.333+
n−m:n	1.111+	Grave Major Second	1.111+
n−2m:n	1.250	Major Third	1.250
n−2m:n−m	1.125	Acute Major Second	1.125

When m has become relatively greater, reaching the magnitude of $n/6$ (Table VI), $n/5$ (Table VII), and $n/4$ (Table VIII), the musical character of the intervals is such that dissonance would be improbable. Differences between these tables will be discussed presently.

Table VI indicates that considerable consonance could be found either if the notes heard were consonant, of themselves, at all regions of the scale, or if the notes happened to be consonant at the particular region in which the primary note was actually produced. The musical effect would be a matter of the notes composing the chord and their absolute pitch. The chord indicated by Table VI is, f'', a'', c''', d''' (modified), f''', since n is 1024 and m is 170 or $n/6$. We found the first three notes of this chord (Table II) and Mayer, presumably, might have had them. If he did, the smoothness of the consonance would not allow beats at

the region of 1024. Its musical character at other regions, such as at c of 128, is not one of consonance but of dissonance. In this region consonance can be found only with greater intervals between the notes in the chord. In terms of interruptions, m must be more than $n/6$ when n is of sufficiently low pitch. Table IV shows that Mayer's experimental value for 128 meets this requirement.

TABLE VI

INTERVALS CONSONANT AT HIGH PITCH

n+m:n+2m	1.142+	Diminished Third	1.137+
n:n+2m	1.333+	Fourth	1.333+
n−m:n+2m	1.600	Minor Sixth	1.600
n−2m:n+2m	2.000	Octave	2.000
n:n+m	1.166+	Augmented Second	1.171+
n−m:n+m	1.400	Diminished Fifth	1.400
n−2m:n+m	1.750	Minor Seventh	1.777+
n−m:n	1.200	Minor Third	1.200
n−2m:n	1.500	Fifth	1.500
n−2m:n−m	1.250	Major Third	1.250

What is true regarding interval and absolute pitch on the one hand and consonance and relative value of m on the other in Table VI is also true in Tables VII and VIII.

TABLE VII

INTERVALS CONSONANT AT INTERMEDIATE PITCH

n+m:n+2m	1.166+	Augmented Second	1.171+
n:n+2m	1.400	Fifth	1.500
n−m:n+2m	1.750	Minor Seventh	1.777+
n−2m:n+2m	2.333+	Minor Tenth	2.400
n:n+m	1.200	Minor Third	1.200
n−m:n+m	1.500	Fifth	1.500
n−2m:n+m	2.000	Octave	2.000
n−m:n	1.250	Major Third	1.250
n−2m:n	1.666+	Major Sixth	1.666+
n−2m:n−m	1.333+	Fourth	1.333+

The chord from Table VII applies almost equally well, since it is based on $m=n/5$, to the three primaries, c'' of 512, e'' of 640, and g'' of 768. The full chord for c'', constructed by means of the values of $n-2m$, $n-m$, n, $n+m$, $n+2m$, is e'-flat, a'-flat, c'', e''-flat, and g''; for e'' it is g', c'', e'', g'', and b''-flat; and for g'' it is b'-flat, e''-flat, g'', b''-flat, and d'''. Table II shows that we found the three notes in the middle of these three chords at Mayer's

stipulated number of interruptions. Their full consonance in the region in which they were used in the experiments and the dissonance of the same intervals lower in the scale support our contention that Mayer may well have judged only consonance.

TABLE VIII

INTERVALS CONSONANT AT LOW PITCH

n+m:n+2m	1.2	Minor Third	1.2
n:n+2m	1.5	Fifth	1.5
n−m:n+2m	2.0	Octave	2.0
n−2m:n+2m	3.0	Twelfth	3.0
n:n+m	1.2+	Major Third	1.2+
n−m:n+m	1.6+	Major Sixth	1.6+
n−2m:n+m	2.5	Major Tenth	2.5
n−m:n	1.3+	Fourth	1.3+
n−2m:n	2.0	Octave	2.0
n−2m:n−m	1.5	Fifth	1.5

The chord from Table VIII ought to apply rather well to the primaries *c* of 128, *c′* of 256, *e′* of 320, and *g′* of 384, on account of the fact that Mayer's values for these tones are approximately $n/4$. Constructed in the same manner as the chords from Table VIII, the full chord for c is *C*, *G*, *c*, *e*, and *g*; for *c′* it is *c*, *g*, *c′*, *e′*, and *g′*; for *e′* it is *e*, *b*, *e′*, *a′*-flat, and *b′*; and for *g′* it is *g*, *d*, *g′*, *b′*, and *d″*. These chords, except for *e′*, contain the notes for the same primaries as they are given in Table II. The abridgment is only slight in the several cases. The note *G* was not heard in the first chord, the note *c* in the second, and the notes *g* and *d* in the fourth. The result for the primary *e′*, the exceptional case, when calculated according to the intervals in Table VII instead of Table VIII is *g*, *c′*, *e′*, *g′*, and *b′*. It is interesting that the only inversion in Table IV should fall at *e′*, 320. The chord for *e′*, reported in Table II, is *c′*, *e′*, *g′*, the middle notes from the chord given above. This chord from Table VII happens to be more appropriate for *e′* than the one from Table VIII. This might have been expected since $m = n/4.4$ is almost as near to the $n/5$ of Table VII as it is to the $n/4$ of Table VIII. The harmonic difference, however, between *b*, *e′*, *a′*-flat and *c′*, *e′*, *g′* is very slight. The former, in this region of the scale, seems to be slightly more consonant than the latter, as it might well be, since it is an interval of nine semitones, whereas the interval from *c′* to *g′* contains only seven semitones. This sole and rather slight discrepancy between the calculated and

the experimentally determined values hardly vitiates our argument that Mayer could have been judging consonance unwittingly. This is especially true on account of the fact that we are using nothing better than approximate values in our calculations.

In conclusion, our argument is simple. Mayer's results, when interpreted musically are a matter of consonance and dissonance. Although absolute rate of interruption varied directly with rate of vibration of the stimulus tones, the relative rate of interruption varied inversely with it. The relatively slow interruptions were sufficiently rapid, in the higher pitches, to produce consonance but could not produce intervals sufficiently great for consonance in the lower pitches. It happens that Mayer found rates suitable for consonance at all the points of the scale from which he chose his stimulus tones. Indeed, the chord produced was the common chord (*C*, *E*, *G*), with some extensions above and below, or a very close approximation to it. The same type of musical perception could, therefore, be employed in all judgments in his experimental series, and we assume that this was the way in which the judgments were actually made. The apparently different rates of interruption are, musically, really identical, if we consider both harmony for itself and the absolute pitch of the chord. Every single experiment in Mayer's whole series of experiments ended when consonance first appeared, and in our experience this stage, for audition, was very much like a single, smooth primary but was not identical with it. Physically, the secondaries ought to have been present; actually, we found some of them but Mayer did not. Unfortunately, it is one set of observations against another, hardly a determinate situation for science.

Summary

(1) Mayer's conditions were not precisely as he reported them. His stimulus-tone was not interrupted by intervals of silence, as he thought, but by changes in intensity.

(2) When we reduced the intensity of our tone, we found that we were unable to obtain an alternation of sound and silence. The tone disappeared in noise before Mayer's rate of interruption was attained.

(3) We found, under Mayer's conditions, that the stimulus-tone was smooth as he reported at his rates of interruption, but

that there were other and new tones present when smoothness was obtained that were not reported by him.

(4) The new tones formed consonant chords at the rates of interruption that Mayer used.

(5) The unity of the clang accounts, we believe, for Mayer's failure to note the new tones.

(6) If Mayer heard the new tones without identifying them his results can readily be explained as the recognition of the smoothness of consonance.

PHYSICAL ASYMMETRIES AND DISORIENTATION

By Frederick H. Lund, Bucknell University

Despite the interest and attention which for centuries have been given to the problem of orientation, a good deal of vagueness still surrounds the major issues. Mystic capacities are still invoked to explain the more complex and less certain features of the process. Writers on the subject are not content merely to speak of adjustments to objects and relations in time and space, but are inclined to feel that, over and above these specific adjustments, there is a more general capacity or orientation sense which controls the individual adjustments. The hope still lingers that we may find an internal supra-sensory faculty with special powers of direction and control. Of such a power we have no direct experience. This being the case, it cannot devolve upon us to prove or disprove its existence, and we should be free to work on the more natural assumption that the adjustments in question depend upon cues registered by the ordinary senses.

Most of the investigation in the past has been concerned with the invertebrates and, more especially, the insects. Very little work has been done on the higher orders, and hardly any in the case of man. The greater perfection of the capacity in the lower forms and the greater opportunity for research have, no doubt, been of chief importance in determining the direction which interest in the problem has taken.

In the case of man the best work has been done on the labyrinthine and equilibrating mechanisms. Here, as in many other instances, the most valuable and illuminating data have been derived from pathological studies and from direct observation of the effects of eliminating the supposedly significant factor. Following this suggestion, the present study was concerned with a series of experiments in which the visual sense, generally recognized as most important for orientation in man, was eliminated as a contributing factor.

Everyone must have vivid impressions of the disturbing effects of blindfolding in such games as blind-man's buff, and the state

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of complete disorientation which develops in the course of the game. The all-important rôle of the visual sense has been emphasized also by recent experiments in aviation. Magnus states that "the exclusion of visual impressions when the aeroplane is passing through mist or clouds makes orientation practically impossible. Often when the aviator emerges from the clouds and can once more see the ground, he finds himself completely disoriented with respect to the earth."[1]

In the experiments reported in this article blindfolded *S*s were required to walk given distances under prescribed conditions. Elimination of the visual sense produced varying amounts of disorientation, expressing itself in a disposition to veer from the desired or expected line of movement. In following up these results interest naturally centered about an attempt to account for the nature of the veering. While the data thus secured may have only indirect bearing upon the problem of orientation, it is hoped, nevertheless, that they may throw some light on the process and possibly indicate the direction which the solution of the larger issues ultimately will take.

Method and Procedure

Experiment 1: Walking straight-ahead. A football field presenting a fairly even and uniform surface of green turf was selected for the experiment. With the aid of a surveyor the field was mapped out as shown in Fig. 1. Small cardboard signs were placed at the points indicated in the diagram. These signs, marking the degrees of deviation from the median line, enabled *E* to plot the course of *S* with a fair amount of accuracy. One hundred twenty-five *S*s, students of psychology, participated in the tests. Before starting, *S* was brought into position at 'O' (see Fig. 1) and asked to orient himself with respect to 'P,' his objective in walking. Upon being blindfolded he would start immediately for 'P' guided from behind for the first few steps in order to insure correct initial orientation. He was permitted to set his own pace. *E*, following close behind, plotted the course of *S* on a specially prepared chart. The limit of the field reached, *S* was requested to make several pivotal turns before being led back to 'O' to repeat the performance. The pivotal turns served to eliminate any possible cues as to the nature of the veering. Six such records were secured for each *S*.

Experiment 2: Walking backward. The bee-line which the *S* attempted to make in walking from 'O' to the limit of the field usually turned out to be a more or less uniform curve to the right or to the left. In view of this, and in view of the consistency characterizing successive performances of the same *S*, it was considered desirable to secure a check on the nature of the deflectional

[1]Rudolph Magnus, Physiology of posture, *Lancet*, 2, 1926, 588.

tendency by having *S* walk backward. If a structural factor were responsible for the deflection, then the disposition to veer should be reversed when *S* walks backward, since this change in position reverses the plane of the body relative to the objective. Except for these variations the procedure was the same as in the first experiment. Seventy-five *S*s who had shown greatest consistency in right-veering and left-veering were selected for this experiment.

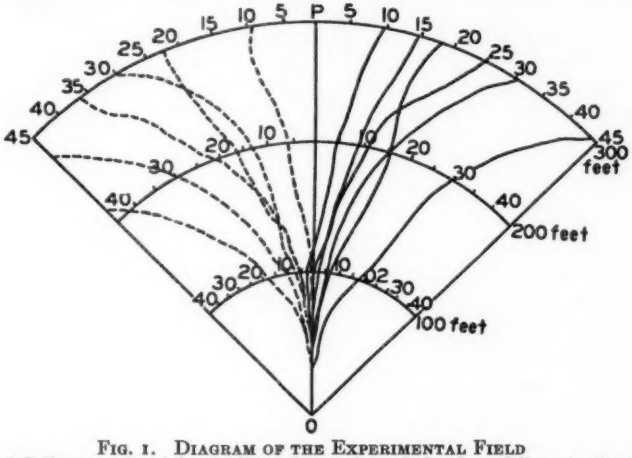

FIG. 1. DIAGRAM OF THE EXPERIMENTAL FIELD
The full lines to the right (walking forward) and the dotted lines to the left (walking backward) indicate the performance of a left-dominant *S* in *Experiments 1* and *2*.

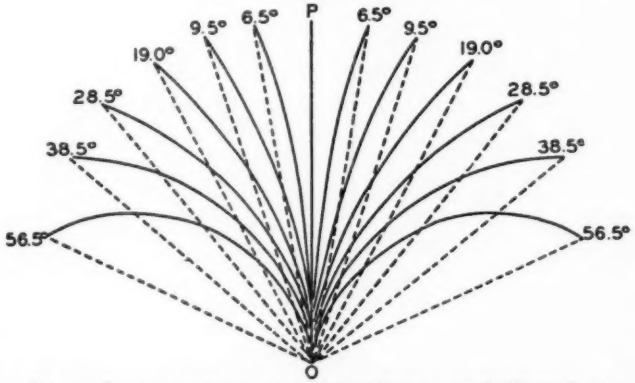

FIG. 2. DIAGRAM OF THE ARCS OVER WHICH THE *S*s WERE LED
The angular deviation of each arc from the line 'O-P' is indicated at the extremity of each arc.

Experiment 3: Walking over prescribed arcs. On the same field 200-foot arcs tangent to the same line were described by radii of 100, 150, 200, 300, 600, and 900 ft. respectively. Six such arcs were drawn to the right and six to the left. Fig. 2 is a diagram of the field and shows the amount of deflection of each arc from the line 'O-P.' Tracing these arcs in liquid lime made them available as courses over which the *S* might be led. Throughout this experiment a mechanical device was interposed between the *E* and the *S* so as to keep the latter on the course and at an appropriate distance from the *E*. *S* was told that he would be led over these arcs and that his problem would be to judge whether he was being taken to the right, the left, or straight-ahead. Starting with the outer arcs and alternating with the straight path, *E* was able gradually to eliminate those arcs in the case of which the deflectional tendency was easily detected; and finally, by a process of elimination was able to determine the approximate norm of deflection of a given *S*. This experiment served as a further check upon the functional dispositions displayed in *Experiments 1* and *2*.

Measurement of Bilateral Symmetry

A preliminary survey of the results showed not only a certain amount of consistency for most *S*s in successive performances in a given experiment, but equal consistency as between experiments. Inasmuch as the most likely explanation of the presence or absence of these functional asymmetries was to be found in some significant structural asymmetry, measurement of all *S*s engaging in the three experiments was undertaken in the following characteristics:

(*1*) *Handedness.* Depending upon dextral usage, the *S*s were classified as dextral, sinistral, or ambidexterous, or, to use the expressions later introduced for the sake of having a common basis for comparison, as *right-dominant, left-dominant,* or *non-dominant.*

(*2*) *Eyedness.* Since some authorities consider eyedness a more significant measure of functional unilateral dominance, in fact, maintain that handedness depends upon eyedness, the Parsons' manuscopic test was used to supplement the measure of handedness.

(*3*) *Length of arms.* The measurement of the length of the *S*s' arms was undertaken because of the relation known to exist between asymmetry in this instance and handedness.

(*4*) *Length of legs.* A measure of the relative length of the two legs was first secured by measuring the distance from the anterior-superior spine to the floor for each leg, *S* meanwhile resting his entire weight on the limb being measured. The uncertainty in some of the point on the ilium from which the measurement was taken made it desirable to secure a check by a different method. The *S* was placed outstretched on a table. A straight position was secured by bringing the feet together in such a way that their juncture was in a line with the median line of the trunk. Two square blocks of wood were then forced with equal pressure against the soles of the feet. The difference in the position of the blocks was taken as a measure of the difference in the length of the legs.

(*5*) *Posture.* It has long been known that the right side of the body may be as different from the left as 'identical' twins may be different. They are, in fact, only imperfect twins. It was believed that superior structural development of a given side would tend to make the individual incline slightly to the

other. In measuring for posture the *S* was asked to bring his heels together and otherwise to assume a natural posture. A perpendicular was then erected behind his back from the point where the heels were in contact. Leaning to the right was interpreted as left-dominance, leaning to the left as right-dominance.

(*6*) *Walking straight-ahead.* This was the first of the experiments described above. Disposition to veer to a given side was taken to mean dominance of the opposite side.

(*7*) *Walking backward.* Since the position of the limbs relative to the line of movement is reversed in this experiment, right veering would have to mean right-dominance, left veering left-dominance.

(*8*) *Walking over arcs.* Predominance of errors on right arcs (see *Experiment 8*) was interpreted as left-dominance, predominance of errors on left arcs as right-dominance.

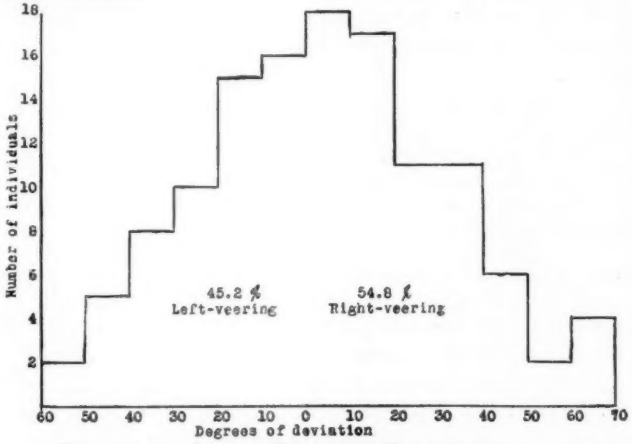

FIG. 3. CHART SHOWING RESULTS OF EXPERIMENTS 1, 2, AND 3
Distribution of 125 *S*s according to amount of deviation in six successive walking performances while blindfolded.

RESULTS

The results are based upon 3542 trials, or walking performances, of 125 *S*s, and upon the data derived from measurement of the structural and functional traits enumerated in the previous section. Upon completion of the first experiment every *S*'s average deflection from the median line was computed from his chart, the terminal point of each performance being made the basis for compution. The results of these computations are shown in Fig. 3. As will be seen, more *S*s veered to the right than to the left. This greater frequency of left-dominance indicated by more frequent right-veering is paralleled by more frequent structural dominance of the left leg, as later measurement showed 39% of

left-dominance, 33% of right-dominance, and 28% of equality. On the other hand, measurement of the arms of the same *Ss* showed 55% of right-dominance, 31% of left-dominance, and 14% of equality. This fact of 'crossed symmetry' (longer arm on one side coupled with longer leg on the other) has been reported by Hasse and Dehner,[2] Guldberg,[3] and others.

Before proceeding to the second experiment the *Ss* were divided into three groups according to the dominant trend of their performance in the first experiment: Group I, 41 *Ss*, right-veering; Group II, 34 *Ss*, left-veering; Group III, 50 *Ss*, in general, forward-moving. Measurement of these groups yielded the following results:

Group I: 78% longer left leg; left leg on the average 0.42 cm. (median 0.52 cm.) longer than the right, with an A.D. of 0.17 cm.

Group II: 83% longer right leg; right leg on the average 0.61 cm. (median 0.63 cm.) longer than the left, with an A.D. of 0.20 cm.

Group III: 65% equality of right and left leg; median gave equality; average favored the left leg by 0.07 cm. with an A.D. of 0.06 cm.

These figures show a striking correspondence between structural and functional dominance. The results of *Experiments 2* and *3* yielded only further confirmation of this relationship, the correspondence being slightly greater than in *Experiment 1*. Groups I and II alone served as *Ss* in the second and third experiments. A complete summary of the results for all three experiments follows.

Walking straight-ahead. Of the *Ss* veering left, indicating *right-dominance*,
83% had longer right leg, exceeding the left on the average by 0.61 cm., with an A.D. of 0.20 cm. (0.63 cm. median measure of the same excess);
77% veered right in walking backward;
83% showed excess errors on left arcs;
75% showed right-dominant posture;
92% were right-handed;
80% were right-eyed.

Of the *Ss* veering right, indicating *left-dominance*,
78% had longer left leg, exceeding the right on the av. by 0.42 cm. with an A.D. of 0.17 cm. (0.52 cm. median measure of the same excess);
80% veered left in walking backward;

[2] C. Hasse and M. Dehner, Unsere Truppen in körperliche Beziehung, *Arch. f. Anat. u. Entwicklungsgesch.*, 16, 1893, 249-256.

[3] G. A. Guldberg, Über die morphologische und functionelle Asymmetrie der Gliedmassen beim Menschen und bei den höheren Vertebraten, *Biol. Centralbl.*, 16, 1896, 806-813.

91% showed excess errors on right arcs;
73% showed left-dominant posture;
84% were right-handed;
53% were left-eyed.

Walking backward. Of the *Ss* veering right, indicating *right-dominance,*
85% had longer right leg;
77% veered left in walking 'straight-ahead;'
71% showed excess errors on left arcs;
69% showed right-dominant posture;
85% were right-handed;
77% were right-eyed.
Of the *Ss* veering left, indicating *left-dominance,*
79% had longer left leg;
80% veered right in walking 'straight-ahead;'
88% showed excess errors on right arcs;
84% showed left-dominant posture;
84% were right-handed;
62% were right-eyed.

Walking over arcs. Of the *Ss* showing excess errors on left arcs, indicating *right-dominance,*
83% veered left in walking 'straight-ahead;'
71% veered right in walking backwards;
67% had longer right leg;
78% showed right-dominant posture;
89% were right-handed;
89% were right-eyed.
Of the *Ss* showing excess errors on right arcs, indicating *left-dominance,*
91% veered right is walking 'straight-ahead;'
88% veered left in walking backward;
79% had longer left leg;
80% showed left-dominant posture;
91% were right-handed;
54% were left-eyed.

Interpretation of Results

The disposition to turn, veer, or 'circle' appears with a high degree of regularity in organisms both high and low in the scale of life. In some it is manifest at all times, while in others, mostly of the higher order, it appears only when the sensory cues normally guiding spatial adjustments have been eliminated. As a *functional asymmetry* this disposition to veer would seem most naturally to call for explanation in terms of some *structural asymmetry.* But, unfortunately, the naturalness or simplicity of an explanation does not always insure its acceptance. There are always those

who are specially alert to suggestions of a more intriguing nature, and who prefer to inject mystery into their account wherever possible. So it has been even in the phenomena under consideration. When confronted with the seemingly needless yet continuous spiralling of paramecia, or the circular course taken by game when hard pressed, or, again, a similar performance on the part of individuals when lost, certain scientists have been inclined to sidestep the simpler and more natural morphological account and to introduce special agencies from the realm of the unobservable.

Asa Schaeffer, Professor of Zoölogy in Kansas University, who has made wide observations on veering tendencies in many species, is convinced that men and animals are possessed of a "spiralling mechanism" or a "steering mechanism that makes them turn in spirals when it gets control of a situation—a sort of 'sixth sense' that most people have never dreamed that they possess."[4] "No forward moving organism," says Schaeffer, "has yet been found that does not move in some form of spiral path when there are no orienting senses to guide it. . . This extraordinary unanimity of observations can hardly be interpreted in any other way than that it is a universal property of moving living matter."[5] Concerning this "spiralling mechanism" he finds "comparatively little if any evolution. . . . The same mechanism operates in man as operates in the amoeba." To avoid being challenged with an outright mysticism, he proceeds to localize his newly discovered agency: "So far as man is concerned the center of mechanism is, of course, in the brain." Possibly fearing that this might lead him into admitting some structural influence, which from the outset he has discarded as "erroneous" and "absurd," he qualifies his statement with the admission that "the effective mechanism may at any given time be located in a single cell in the human brain."

These statements are made by Schaeffer at the conclusion of his work on *Spiral Movement in Man*, which had been preceded by a study of similar movement in the case of lower forms. In his experiments on man he recorded the paths of blind-folded *Ss* while walking and while swimming. On the basis of his studies, he feels justified in concluding that "the spiral turns are not due to asymmetries of the legs or other parts of the locomotor organs"

[4]For a summary of Schaeffer's results see *Literary Digest*, Aug. 11, 1928.
[5]Asa Schaeffer, Spiral movement in man, *J. Morph. and Physiol.*, 45, 1928, 297 ff.

(pp. 349, 375, 376, 393). This categorical statement is presented in spite of the fact that no measurements were made, or, at least, recorded, and in spite of the fact that his results are suggestive of a structural interpretation. He finds, for instance, just as in our own experiment, that right-turning predominates in walking, and, again, that left-turning predominates in swimming. But more frequent right-turning in walking is correlated with the fact (apparently known to Schaeffer, but for some reason overlooked) that a majority of persons have the left leg longer than the right. Again, more frequent left-turning in swimming is correlated with more frequent longer and stronger right arms than left.

In view of these facts there is reason to believe that evidence of a structural influence would have been at hand had measurements been made. In any event, we can hardly regard our own results as indicative of anything but a high degree of interdependence of functional and structural asymmetries. That 80% of the *S*s should show correspondence between structural and functional dominance is hardly a chance matter; and what is more to the point, this correspondence was increased when consideration was limited to those *S*s who showed consistency in more than one experiment. Added corroborative evidence was found in the fact that degree of structural dominance is correlated with degree of functional dominance. This became apparent when the *S*s were grouped according to amount of structural dominance of a given side:

	Structural disparity in leg-length (cm.)	Median deflection in walking (deg.)
Group A	0–0.3	14
Group B	0.4–0.6	25
Group C	0.7–0.9	25
Group D	1.0–1.3	27

But what of the 20% who failed to show correspondence between structural and functional dominance? And what, indeed, of the fact that no S veered in precisely the same manner in successive trials? Since the structural traits must have been constant, the important variant or variants must be found in the sphere of function. The difficulty really is in the matter of control and in the matter of keeping conditions constant throughout the experiment. One cannot always be sure of the full coöperation of the *S*. But even the *S* who tries his best cannot be expected in an experiment of this kind to keep his mental set more than relatively con-

stant over so long a period of time. There are always sensory elements of internal and of external origin which necessarily vary from trial to trial and which consciously or unconsciously influence the mental set and change the conditions throughout the motorium. But the importance of these sensory elements and of the mental set controlling the spatial adjustments brings us back to the problem of orientation.

Disorientation

Most blindfolded *Ss* after walking 300 feet find themselves facing in a direction at variance with their expectations, and after walking 500 feet many are completely disoriented, the veering process having resulted in a reversal of the line of movement. If, indeed, the individual possessed a special sense of direction, what sort of a sense could this be to allow for such anomalies of behavior? As already indicated, we prefer to work on the assumption that no special sense is involved. There is usually more willingness to admit this in the case of human behavior than in the case of some of the lower orders. But even in the lower animals the best opinion is inclined to rule out the possibility of a special sense for which there is no known anatomical basis. Rabaud, an eminent authority in the field, has recently brought together the results of the best experimental work on the subject, including investigation of the more unusual performances of insects and of homing and migratory birds. By way of conclusion he remarks that "the facts based upon well-conducted experiments or observations constantly reduce the phenomena of orientation and of place recognition to processes of memory bringing into play the organs of the ordinary senses."[6]

As for the senses involved in our own spatial adjustments, the visual is clearly dominant, the auditory and kinaesthetic assuming lesser rôles. In orientation of position and posture Magnus[7] considers the tactile sense almost as important as the visual and kinaesthetic. According to this author, the importance of the cerebellum and the labyrinth for attitudinal and righting adjustments has been greatly exaggerated.

But allowing that these senses and the impressions registered by them are adequate to account for orientation, how shall we

[6]E. Rabaud, *How Animals Find Their Way About*, 1928, 1-123.
[7]Magnus, *op. cit.*, 588 f.

account for the relatively stable and definite mental content usually described as our 'sense of direction'? How account for the equally definite experience of 'being turned around' with its resultant shift in getting again orientated?

The writer secured the coöperation of 5 *S*s in an attempt to gain introspective data which might throw some light on this question. Introspective reports were called for immediately following the removal of the blind-fold in situations in which the surroundings and the line of movement were not those expected. Analysis of these reports indicated that two forms of awaremess were readily distinguishable by the *S*: awareness of the relation between bodily position and the environmental lay-out, and awareness of the relative position of objects within this lay-out. Taken together, these forms of awareness constitute a mental configuration or mental set described by the individual as his "sense of direction." This mental set is relatively constant, and it must be so in order that objects within the field may shift without disturbing the contextual lay-out of the field.

The experience of being 'turned around' results when sensory impressions are registered which cannot readily be fitted into the general scheme or which belong to a context different from the one in control. The momentary confusion occasioned by these unexpected impressions institutes a process of reorganization which results in an elaboration of a new mental set with the attendant feeling of being 'turned right' again. Such, at least, seems to be the only interpretation possible with the facts at present available.

Summary

Exclusion of the visual sense, which usually guides spatial adjustments, results in varying amounts of disorientation, expressing itself in a disposition to veer or in a disparity between actual and intended movement. The blindfolded *S* attempting to walk in a straight line typically veers to the right or to the left. The direction of the veering is hardly a chance matter, since the same *S* placed under the same conditions tends to veer in the same direction in successive trials. Consistent right-veering may be regarded as a case of functional dominance of the left side of the body, consistent left-veering as a case of functional dominance of the right side. We ascribe such (functional) unilateral dominance

to some structural unilateral dominance, and present the following facts in support of our interpretation.

(1) The consistency of right-veering or left-veering of the same *S* in successive trials,

(2) The reversal of the veering process when walking backward, in the case of four out of every 5 *S*s.

(3) The difficulty experienced by *S*s in distinguishing the direction of the veering when led over a course corresponding to the normal disposition to veer.

(4) The correspondence between functional and structural dominance (superior structural development of the right leg in left-veering subjects and *vice versa*) in four out of every 5 *S*s.

(5) The increase in correspondence between structural and functional dominance when only those *S*s are considered who showed consistency in all performances.

The individual's 'sense of direction' may best be conceived as a mental set or a contextual form of awareness pertaining to the relation between bodily position and the environmental lay-out. The experience of being 'turned around' results when sensory impressions are registered which are out of accord with existing mental context. This institutes a process of reorganization which results in a contextual shift and a feeling of being 'turned right' again.

A STUDY OF THE FACTORS WHICH CAUSE INDIVIDUAL DIFFERENCES IN THE SIZE OF THE FORM-FIELD

By C. E. Ferree, G. Rand, and M. M. Monroe[1]

In recent years papers have been published by the senior authors showing the factors which influence the extent and shape of the form and color fields in a given individual, and describing the apparatus and controls which are needed to eliminate or minimize the effect of these factors and to secure accuracy and precision of result in perimetry.[2] In two papers by the present writers a method of treatment of the remainder of the factors was outlined,[3] namely, those which vary from individual to individual. It was also indicated in these papers that as fast as possible data would be given showing the influence of these factors on the size of the form and color fields. It is the purpose of the present paper to show the relation of three of them, errors of refraction, age and sex, to the size of the form-field for a stimulus subtending a visual angle of 1°. The influence of these factors can not be shown directly by experimental variation, as was the case with the factors which vary for a given individual. The procedure used has been to collect the data under standard conditions of control for a group of non-pathological individuals sampled to include the factors under investigation, and to determine their influence by statistical study so far as was possible in the situation presented.

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[1]Ferree and Rand, Research Laboratory of Physiological Optics, Wilmer Ophthalmological Institute, Johns Hopkins Medical School; M. M. Monroe, Graduate School of Medicine, University of Pennsylvania.

[2]C. E. Ferree and G. Rand, Chromatic thresholds of sensation from center to periphery of the retina and their bearing on color theory, *Psychol. Rev.*, 26, 1919, 16-41, 150-163; The absolute limits of color sensitivity and the effect of intensity of light on the apparent limits, *ibid.*, 27, 1920, 1-24; The limits of color sensitivity: effect of preëxposure and surrounding field, *ibid.*, 377-398; A new laboratory and clinic perimeter, *J. Exper. Psychol.*, 5, 1922, 46-67; The effect of variations of the intensity of the perimeter arm on the determinations of the color fields, *Psychol. Rev.*, 29, 1922, 457-473; Perimetery: variable factors influencing the breadth of the color fields, *Amer. J. Ophthal.*, 5, 1922, 886-895; The effect of size of stimulus and brightness of preëxposure and surrounding field on the extent and shape of the color fields, *J. Gen. Psychol.*, (in press).

[3]C. E. Ferree, G. Rand, and M. M. Monroe, Studies in perimetry: I. Preliminary work on a diagnostic scale for the form field, *Amer. J. Ophthal.*, 9, 1926, 95-104; II. Preliminary work on a diagnostic scale for the color fields, *ibid.*, 12, 1929, 269-285.

The results obtained by such a procedure are not so clear-cut and incisive as are those obtained by the experimental method, but the procedure used is the only one available in cases in which experimental analysis is not possible.

The fields were taken on the Ferree-Rand perimeter with a strict observance of all the precautions prescribed for the use of this perimeter. With this instrument all of the external conditions of the test can be held constant and reproduced at will. The fields were determined with a white stimulus on a black background. The coefficients of reflection of stimulus and background were respectively 78% and 4%. The illumination at every point in the field was kept constant at 7 foot-candles. In every instance the refractive condition of the eye was carefully determined and an ophthalmoscopic examination made. No cases showing a pathological condition, however slight, were included in the series. Ample rest periods were allowed between observations, and care was exercised that no field should be taken when the *S* was suffering from general fatigue or was otherwise unfit for accurate work. All determinations were carefully checked, but the *S* was not given extensive preliminary training. It was not the purpose of the writers to make examinations for the determination of the influence of the factors in question under conditions differing in this respect from those which obtain in the ordinary practice of perimetry. The results obtained may, therefore, be considered a fair example of the effects that may be expected in an average group of untrained *S*s who show no pathology.

Fields were determined in eight meridional quadrants (0°, 45°, 90°, 135°, 180°, 225°, 270°, and 315°). The eyes examined included 75 cases of hyperopia and hyperopic astigmatism, 30 cases of myopia and myopic astigmatism; 40 cases of presbyopia; 5 cases of mixed astigmatism; and 50 cases showing no error of refraction or an error no greater than 1 diopter of hyperopia or ¼ diopter of hyperopic astigmatism. This last group contained no cases of myopia or myopic astigmatism of a detectable amount. For convenience of treatment it will be referred to as emmetropic. The *S*s ranged in age from 8 to 56 years. There were 84 males and 116 females.

In making comparisons of the kind considered in this paper a single value or index is needed to represent size of field. Two types of index have been discussed and used in our former papers: the average breadth of field in the eight meridional quadrants; and the area of field as measured with a planimeter on a map drawn to a predetermined scale. The former of these has been selected for use in the present paper.

Graphic representations of results are given in Figs. 1-3. In Fig. 1 average breadth of field in degrees is plotted against frequency of occurrence. The total range of average breadth of field for the 200 cases was from 59.9°–74.4°. For the purpose of grading and representation in the plot the cases were separated into 9 groups, each group covering a range of 2°. The cases are designated with regard to condition of refraction according to a key shown in the chart.

The results represented in Fig. 1 may be summarized as follows:

(1) In general the emmetropes and hyperopes have the wider fields; the myopes and presbyopes, the narrower fields.

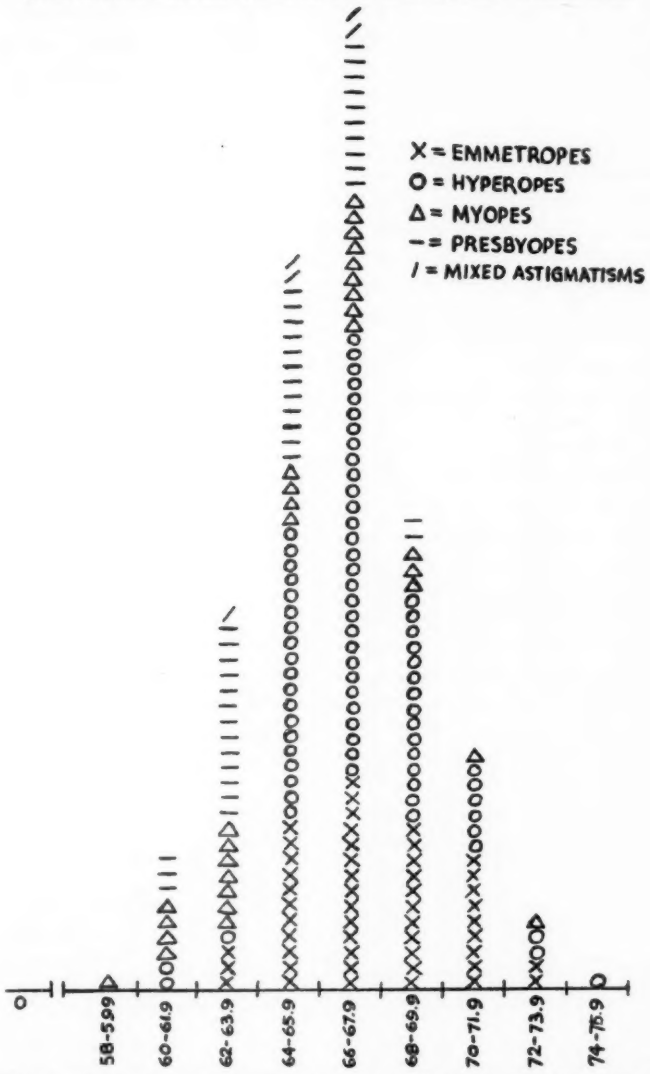

FIG. 1. SHOWING THE DISTRIBUTION OF 200 NON-PATHOLOGICAL CASES
The *Ss* were selected to include cases of emmetropia, hyperopia, myopia, presbyopia, and mixed astigmatism. The distribution is based on the average breadth of field in the eight principal quadrants. Breadth of field in degrees is plotted on the horizontal coördinate and frequency of occurrence on the vertical coördinate.

(2) Ranked from narrowest to widest, the percentage of cases falling in each of the 9 groups is as follows: 0.5, 4.5, 12.5, 24, 32, 15.5, 8, 2.5, and 0.5.

(3) The myopes constitute all of the first group, 44.4% of the second group; 28% of the third group; 8.4% of the fourth group; 14.5% of the fifth group; 9.7% of the sixth group; 6.2% of the seventh group; 20% of the eighth group; and none of the ninth group. The presbyopes form no part of the first, seventh, eighth, and ninth groups; 33.3% of the second group; 52% of the third group; 25% of the fourth group; 14.5% of the fifth group; and 6.5% of the sixth group. Emmetropes are not found in the first, second, and ninth groups; they form 12% of the third group; 23% of the fourth group; 22.6% of the fifth group; 35.5% of the sixth group; 56.2% of the seventh group; and 40% of the eighth group. No hyperopes are found in the first group; they form 22.2% of the second group; 4% of the third group, 39.6% of the fourth group; 46.8% of the fifth group; 48.4% of the sixth group; 37.5% of the eighth group; and all of the ninth group.

(4) The distribution of average breadth of field for the entire group of 200 cases, it is interesting to note, is of the type frequently found when a large number of measurements is made of some physiological or psychological characteristic. That is, the bulk of the cases cluster around a median value, fewer and fewer occurring as this value is receded from. Whether or not the frequency distribution of field size in non-pathological cases will be found to fit the normal or Gaussian curve as many other physiological traits have been found to do can not be determined until a much larger number of cases has been investigated. The approximation is as close as can be expected, however, with 200 cases.

In Fig. 2 is given a graphic representation of the results for each refraction group considered separately.

In the consideration of these graphs the question of causes of relation of error of refraction and size of field naturally arises. The following possibilities are suggested: (a) Unclearness of the image in the more extreme cases of refraction defects. The consideration of this possibility is complicated, however, by the lack of knowledge for the cases in question of the refraction situation for the peripheral portions of the field. In our more detailed and scientific work in the study of the peripheral field we have been accustomed to determine its refraction. In a study of the kind

made here such a detailed specification of condition of refraction was quite impossible. (b) The placement of the image on the retina. For example, in cases of hyperopia due to deficient curvature of the cornea one might expect as a phenomenon of refraction that the image for a given excentricity of stimulus in the field would be formed nearer to the center of the retina than for the same position of the stimulus in cases of emmetropia and myopia. The tendency of this factor would be to widen the field for hyperopia. Conversely, in cases of myopia due to excessive curvature

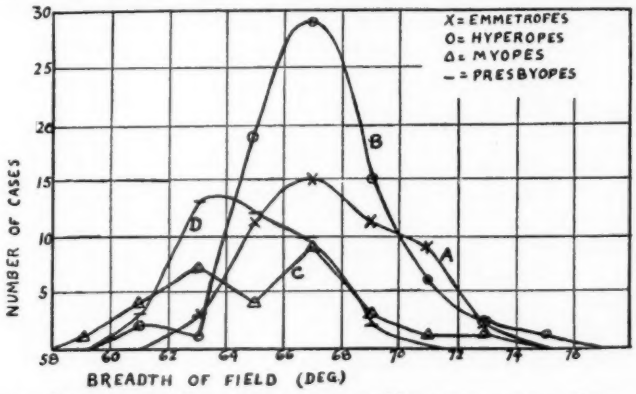

FIG. 2. SHOWING THE DISTRIBUTION OF 200 NON-PATHOLOGICAL CASES FOR EVERY REFRACTION GROUP

Curve A shows the distribution for the emmetropes; B, for the hyperopes; C, the myopes; and D, the presbyopes. The distributions are based on the average breadth of field in the eight principal meridional quadrants.

of the cornea, one might expect for a given excentricity of the stimulus in the field that the image would be formed relatively farther from the center of the retina. The influence of this factor would tend to narrow the field in myopia. (c) Characteristic differences in shape of eyeball, in degree of development and sensitivity of the nervous structure and in health of the eye. (d) Age as a dominant factor in presbyopia. With the exception of (d) the data presented in this paper do not justify any serious attempt at a differential evaluation of the possibilities suggested. It may be noted, however, that the sizes of field obtained in the greater number of cases of hyperopia and myopia are in the direction that might be expected from factor (b), curvature of cornea.

Table I shows for the entire group of 200 cases and for the different re fraction groups, the average value of average breadth of field, the standard deviation (σ), the coefficient of variability ($100\sigma/M$), the range for each group, and the range for the middle 50% of cases in each group. It is seen in this table that the averages are larger for the emmetropic and hyperopic groups; and that the scatter is greatest for the myopic and least for the presbyopic group.

TABLE I

SHOWING FOR THE ENTIRE GROUP OF 200 NON-PATHOLOGICAL CASES AND FOR EACH REFRACTION GROUP, THE AVERAGE VALUE OF AVERAGE BREADTH OF FORM-FIELD, THE STANDARD DEVIATION (σ), THE COEFFICIENT OF VARIABILITY ($100\sigma/M$), THE RANGE FOR ALL CASES IN EACH GROUP, AND THE RANGE FOR THE MIDDLE 50% OF CASES

Group	Average value of average breadth of form field	σ	$\frac{100\sigma}{M}$	Range of size of field					
				Total number of cases			Middle 50 per-cent of cases		
				Limiting values	Deg.	%*	Limiting values	Deg.	%*
Entire group (200 cases)	66.45°	2.62	3.9	74.4–59.9	14.5	24	68.1–64.6	3.5	5
Emmetropes (50 cases)	67.59°	2.56	3.8	73.0–62.9	10.1	15	69.8–65.3	4.5	7
Hyperopes (75 cases)	67.1°	2.57	3.8	74.4–60.6	13.8	20	68.3–65.6	2.7	4
Myopes (30 cases)	65.14°	3.10	4.8	72.0–59.9	12.1	19	67.0–62.9	4.1	6
Presbyopes (40 cases)	64.85°	2.14	3.3	69.1–61.0	8.1	13	66.1–63.3	2.8	4

*In computing the percentages for range the average value of the group in question was used as the base.

Tables II and III have been compiled to show the relation between age and average breadth of form-field in the eight principal quadrants. The range of size of field is treated in 5 groups of 4° each, the range of age in years in 6 groups of 10 years each. The rows of Table II show the frequency of occurrence of each group of size of field for each age-group; the columns, the frequency of occurrence of each age group for each size of field. In Table III is shown the percentage of occurrence of each group of size of field for each age group.

An inspection of these tables shows that there is some tendency for size of field and age to vary concomitantly, the larger fields occurring at the younger ages. The correlation between age and size of field computed by the product-moment formula of Pearson for linear relationship is -0.31 ± 0.04. There is, however, a slight non-linearity of relationship shown in the table, due to a pronounced shift towards smaller fields near 40 years of age. That is, above 40 years the mode occurs in group 66–69.9° (average 68°), while below 40 years the mode occurs in the group 62–65.9° (average 64°). Between 30 and 40 years a transitional stage is seen, where there is more nearly an equal number of cases

in the groups whose averages are respectively 68° and 64°. This shift of the mode downward at age 40, and the transitional stage between 30 and 40, will be shown in a later paper to occur also for the color fields. The average size of field for the age-group 10-19 is 67°; for the age group 20-29 is 67.5°; for the age group 30-39 is 66.8°; for the age group 40-49 is 64.8° and for the age group 50-59, 64.7°.

TABLE II

SHOWING THE NUMBER OF CASES IN EACH GROUP OF SIZE OF FORM-FIELD FOR EVERY AGE-GROUP

Age	Average breadth of form-field in degrees					Total
	58–61.9	62–65.9	66–68.9	70–73.9	74–77.9	
0-9			1			1
10-19		14	28	5		47
20-29	4	19	36	12	1	72
30-39	2	10	17	4		33
40-49	4	16	10			30
50-59		14	3			17
TOTAL	10	73	95	21	1	200

TABLE III

SHOWING THE PERCENTAGE OF CASES IN EACH GROUP OF SIZE OF FORM-FIELD FOR EACH AGE-GROUP

Age Group	Average breadth of form-field in degrees					Total
	58–61.9	62–65.9	66–69.9	70–73.9	74–77.9	
0-9			0.5			0.5
10-19		7.0	14.0	2.5		23.5
20-29	2.0	9.5	18.0	6.0	0.5	36.0
30-39	1.0	5.0	5.0	2.0		16.5
40-49	2.0	8.0				15.0
50-59		7.0	1.5			8.5
TOTAL	5.0	36.5	47.5	10.5	0.5	100.0

In Fig. 3 the data have been plotted to show separately the distribution of cases below and above 40 years of age. Curve A gives the distribution of the 153 cases below 40 years of age; Curve B the distribution of the 47 cases above 40 years of age. For ease of comparison the data for Curve B have been re-plotted in Curve C on a magnified scale. The factor of magnification used was 3.25, the ratio of the number of cases in the two groups.

It may be further noted that no very large fields (more than 70°) occur above the age of 40, and no very small fields (less than 62°) below the age of 20. These characteristics also are in general found for the color as well as the form-fields.

The narrower fields of the presbyopic group are probably largely a phenomenon of age rather than an effect of presbyopia. That is, presbyopia, loss of sensitivity, and the general failure of all the powers of the eye are in the main coördinate rather than inter-dependent phenomena of age. In the more advanced cases of presbyopia, however, the defective focussing for near seeing due

to loss of accommodation doubtless exerts a direct influence on the results of perimetry when the stimulus is viewed at a distance of 33 cm. Other age phenomena, extraneous to the functional changes in the sensory mechanism, which may exert an influence on the perimetric results are opacities of the cornea, lens and vitreous; changes in pupil size, etc.

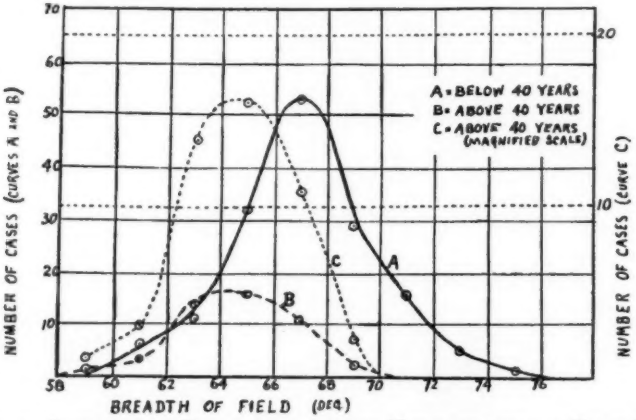

FIG. 3. SHOWING THE DISTRIBUTION OF THE NON-PATHOLOGICAL CASES BELOW AND ABOVE 40 YEARS OF AGE

Curve A shows the distribution of 153 cases below 40 years of age, Curve B the distribution of 47 cases above 40 years of age. Curve C shows, for comparative purposes, the cases above 40 years of age plotted on a magnified scale, the factor of magnification being 3.25—the ratio of the number of cases in the two age-groups.

The results have further been studied to show what relation, if any, exists between sex and the size of the form field. The 200 cases studied included 84 males and 116 females. The median value of size of field rated in terms of average breadth is 66.4°. For the males the average breadth of field for 36 cases or 43% of the number tested falls above, and for 48 cases or 57% of the number tested, below this value; for the females, the average breadth of field for 64 cases or 55% of the number tested falls above, and for 52 cases or 45% of the number tested, below this value. The average breadth of field for the males is 66.04°; for the females 66.75°.

Table IV gives the number of cases of each sex tested in the various refraction groups and the average breadth of field in

degrees for each. It is seen that the averages for the females are slightly but consistently larger than for the males in each group.[4] The difference is too small to be significant in practical perimetry, but it represents a possible point for further study. Whether it represents an actual difference in spread of sensitivity is problematic. The result may be due to other causes. Among these may be mentioned difference in facial conformation: the deeper set eyes, the more prominent nose, overhanging brows, etc. of the males. Some information as to the importance of the factor could be obtained perhaps from a study of the fields for the two sexes meridian by meridian. Sex differences and their analyses will be made the subject of more extensive study, should the data obtained in later work show that the point is of sufficient importance.[5]

TABLE IV

SHOWING THE NUMBER OF CASES OF EACH SEX STUDIED IN EVERY REFRACTION GROUP AND THE AVERAGE VALUE OF AVERAGE BREADTH OF FIELD

Refraction Group	Sex	Number of Cases	Average Value of Average Breadth of Field
Emmetropes	M	22	67.42°
	F	28	67.73°
Hyperopes	M	28	66.42°
	F	47	67.54°
Myopes	M	12	64.98°
	F	18	65.25°
Presbyopes	M	17	64.54°
	F	23	65.09°
Mixed Astigmatisms	M	5	65.50°
	F	0	

In conclusion it may be stated that, (1) Errors of refraction are an important factor influencing the variability of size of the form-field from individual to individual. (2) After 40 years, age is also an important factor. Earlier than this the effect of age is negligible. (3) Sex apparently is not a factor of significant importance.

[4]In this connection the following quotation taken from a letter written to one of the writers by Dr. Henry H. Donaldson of the Wistar Institute of Anatomy and Biology under date of March 26, 1926, is suggestive: "In studying the albino rat we have found the eye bulbs of the female to be heavier than those of the male of like body weight, and the area of the cross section of the optic nerve to be also greater in the female. This last would possibly suggest a slightly greater extension of the retina in the female."

THE ANALYSIS AND SYNTHESIS OF BURNING HEAT[1]

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The studies of heat made during the past third of a century have definitely shown: (1) that the experience is aroused by the stimulation of near-lying cold and warm spots;[2] (2) that it is a simple, unique quality;[3] (3) that it has no qualitative resemblance to either warmth or cold;[4] (4) that pain is not a necessary or intrinsic component;[5] and (5) that the quality of heat lies in the pressure-pain continuum,[6] closer to pressure than to pain.[7]

The early investigators used areal stimuli and temperatures that were adequate for warmth and cold (normal and paradoxical), and frequently for pain. As a consequence, the authors were at odds regarding the ultimate

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[1]From the Psychological Laboratory, University of Illinois. The experiments were performed during the Summer Session of 1928.

[2]Torsten Thunberg, Förnimmelserna vid till samma ställe lokaliserad, samtidigt pagaende köld- och varmerenting, *Upsala Läkaref. Förh.*, 1, 1896, 489 ff. Cf. also W. A. Nagel's *Handbuch der Physiologie des Menschen*, III, 1905, 707.

Sydney Alrutz, Om förnimmelsen 'hett,' *Upsala Läkaref. Förh.*, 2, 1897, 340-359; The sensation 'hot,' *Mind*, 141-144; Studien auf dem Gebiete der Temperatursinne, II. Die Hitzeempfindung, *Skan. Arch. f. Physiol.*, 10, 1900, 340-352.

Friedrich Kiesow, Zur Psychophysiologie der Mundhöhle, *Phil. Stud.*, 14, 1898, 567-588; Zur Analyse der Temperaturempfindungen, *Zsch. f. Psychol.*, 26, 1901, 231-246.

F. Cutolo, Jr., A preliminary study of the psychology of heat, this JOURNAL, 29, 1918, 442-448.

J. H. Alston, The spatial condition of the fusion of warmth and cold in heat, *ibid.*, 31, 1920, 303-312.

E. B. Twitmyer and S. W. Fernberger, A demonstration heat grille, *ibid.*, 38, 1927, 119.

N. C. Burnett and K. M. Dallenbach, The experience of heat, *ibid.*, 38, 1927, 418-431; Heat intensity, *ibid.*, 40, 1928, 484-494.

W. B. Gritman and K. M. Dallenbach, The formula for the intensive gradation of heat, *ibid.*, 41, 1929, 460-464.

[3]Alrutz, *opp. citt.*; Cutolo, *op. cit.*, 445 ff.; Alston, *op. cit.*, 311; Burnett and Dallenbach, *op. cit.*, 423.

[4]Alrutz, *opp. citt.*; Cutolo, *op. cit.*, 445, 447; Burnett and Dallenbach, *op. cit.*, 420 ff.

[5]Alrutz, *opp. citt.*; Cutolo, *op. cit.*, 447 ff.; Alston, *op. cit.*311; Burnett and Dallenbach, *op. cit.*, 420, 423.

[6]Cutolo, *op. cit.*, 445 ff.; Alston, *op. cit.*311; Burnett and Dallenbach, *op. cit.*, 423.

[7]Burnett and Dallenbach, *loc. cit.*

nature of the experience aroused. Thunberg and Kiesow held to the view that it was a complex, *i.e.* a fusion of the cutaneous qualities; Alrutz, who ignored the adventitous pains, that it was unique and further unanalyzable.

The more recent investigators worked with punctiform[8] as well as areal stimuli,[9] and with temperatures that were selected so as to avoid the distracting influence of pain. They did not succeed in avoiding the concomitant arousal of pain with every observer in every experiment, but they did succeed often enough to establish the truth of Alrutz's contention. They obtained at times warm heats, cold heats, and burning and painful heats (just as it is possible with the proper stimuli to obtain warm sweets, cold sweets, and burning and painful sweets), but at other times, and most frequently, the quality of heat stood alone (as does the quality of sweet). When their stimuli were totally inadequate to ache, pain, and paradoxical cold, but adequate to warmth and normal cold, they obtained reports and introspective descriptions which clearly indicated that heat was a simple and unanalyzable quality lying in the pressure-pain continuum.

All the investigators reported, at times, the experience of burning or painful heat. It is probable, in the light of the recent experiments, that these experiences are merely admixtures of heat and pain; but we cannot definitely say so before subjecting them to descriptive analysis.

Object. The object of the present study was to describe the experience of burning heat. Our problem was two-fold: we had first the analytic problem in which we sought to determine, by introspective analysis, the qualities involved in the experience; and secondly, the synthetic problem by which we sought to test the accuracy of our analysis by synthesizing the experience, for, as Bentley says, "no better test of the accuracy of an analysis is possible than the reinstatement of the whole through synthesis of the products of dissection."[10]

Method and Procedure

Our study was divided into five series of experiments: Series I, a preliminary series, taken to determine our *O*'s limens for heat; Series II, synthetic heat; Series III, burning heat; Series IV, synthetic burning heat; and Series V, control series. Though the series are reported separately, Series II, III, and IV were actually conducted in haphazard order. The advantages of this procedure are: (1) it enabled the *O*s to compare, without knowing

[8]Cutolo, *op. cit.*, 443-446; Alston, *op. cit.*

[9]Cutolo, *opp. cit.*, 446 f.; Burnett and Dallenbach, *opp. citt.*; Gritman and Dallenbach, *op. cit.*

[10]I. M. Bentley, The synthetic experiment, this Journal, 1900, 405.

it, the experiments of the different series; (2) it enabled us to judge more accurately their results; (3) it enabled *E* to make more rapid adjustments of the apparatus and thus to run off the experiments more rapidly; and (4) it enabled *D* to work without knowledge of the special conditions of the given experiments.

Apparatus. A temperature grill, identical with the one used by Burnett and Dallenbach,[11] was used in the present study. It consists of two systems of copper tubes bent and interlaced so as to form a grid or grill. Water was forced through the systems by the pressure of the University mains. The temperature of each system was controlled by passing the water through copper coils immersed in warm or cold water baths, and by controlling the water's rate of flow through the system. If the temperature of the warm system was too high, the rate was increased; if too low, the rate was decreased; and conversely with the rate in the cold system.

A small electrical binding post was soldered to each system for the purposes of the experiments in Series IV and V.

The temperature of the experimental room varied during the course of the experiments between 30° and 32°C. The *O*s found it uncomfortably warm, but, unfortunately, it could not be changed. The high temperatures did not, as we found, materially affect the results; they at most merely raised the *O*s' physiological zeros.

Observers. Our *O*s were Mr. E. E. Anderson (*A*), a senior in the University; Mr. J. G. Jenkins (*J*), an instructor in psychology; and the senior author (*D*). *A* was altogether untrained in introspection, and *J* in the observation of heat. They worked without knowledge of problem or method. *D* had served as *O* in two studies on heat, and worked in the present study with knowledge both of problem and procedure. He was not, however, informed of the progress of the investigation, and did not know the time-order of the different experimental series. The *O*s observed 5 days a week from 1-2 P. M.

Instructions. The following instructions were given to the *O*s at the beginning of every experimental hour.

"At the signal 'Now,' place the ventral (dorsal) surface of your right (left) forearm upon the grill. Relax, and keep the pressure of your arm as constant as possible. Remove your arm at the second signal 'Now,' and write your report."

Procedure. After *E* had brought each system of the grill to the desired temperature—and had in Series IV and V properly adjusted the electrical current— he called the *O*s, who were waiting in the experimental room, one at a time. *O* was given specific instructions regarding the arm and surface to be stimulated, seated himself beside the grill, and at the signal 'Now' placed his arm upon it. *E* gave the second signal 10 sec. later, *O* removed his arm and withdrew to write his introspective report, while the next *O* was taken in turn.

We gave the *O*s 8 experiments, on the average, during the course of a single hour. The number varied, however, from 5 to 11, depending upon the ease or

[11] *Op. cit.*, 419.

difficulty that *E* experienced in bringing the cold and warm systems to the desired temperature. None of the four experimental surfaces (the ventral and dorsal surfaces of the right and left arms) was stimulated more than once in 20 min.

The *Os*, in accordance with instructions, relaxed the muscles of the arm stimulated so that its whole weight bore upon the grill. This precaution of keeping the pressure of the arm on the grill as constant as possible from experiment to experiment was taken because, according to the results of Burnett and Dallenbach, the intensity of the heat experience is "conditioned in part by the pressure of the arm upon the grill."[12]

Series I. Preliminary Experiments

We first determined the *Os*' limens for heat, *i.e.* the warm temperatures that in half the cases aroused heat, so that we might, in the synthetic experiments which were to follow, use warm temperatures that were inadequate to heat and pain. Since the earlier experimenters did not determine the limens for their *Os*, one cannot tell whether or not the warm stimuli used in their experiments were adequate for heat and pain. It has generally and incorrectly been assumed that 45°C. is the critical temperature, and that values between this temperature and physiological zero arouse, if anything, only experiences of warmth.[13]

Procedure. We put the copper coils of both systems in the warm water bath. By adjusting the rate of the flow of the water, we brought the systems to the same temperature, 37°C., and then raised the temperature, by steps of 2°, to 45°C. Every *O* gave 2 reports at every step—10 reports in all.

Results. *J*'s limen for heat stood at 43°C., *A*'s and *D*'s at approximately 45°C. We consequently worked in the synthetic experiments with temperatures below these limits.[14]

Series II. Synthetic Heat

We reproduced, in Series II, the earlier synthetic experiments on heat—eliciting the experience of heat with warm and cold stimuli that are themselves inadequate to ache, pain, and paradoxical cold—not in the hope of gaining anything new, but for two other reasons: (1) to set off, by contrast as it were, the experiences of heat aroused (in Series III and IV) by stimuli ade-

[12]Burnett and Dallenbach, *op. cit.*, 425. Bishop's heat grill (H. G. Bishop, An improved heat grill, this Journal, 38, 1927, 648 f.), which, in our opinion, is the best that has been devised, would have insured a constant pressure and obviated this difficulty.

[13]Burnett and Dallenbach had one *O* (*op. cit.*, 424 f.) who reported heat every time he was stimulated with a temperature of 43°C. In a series of supplementary experiments they found that this *O*'s critical temperature was below 43°—the exact point was not, however, established.

[14]These critical temperatures are probably higher, because of the temperature of the experimental room (which varied between 30°-32°C.), than would have obtained under more favorable and usual conditions.

quate to pain; and (2) to obtain descriptions of heat aroused by stimuli inadequate to pain, that could for comparative purposes be used as norms.

Procedure. We varied the temperature of the cold system of the grill from 9°-25°C. for all the *Os*, and of the warm system from 40°-42.5° for *J*, and from 38°-44° for *A* and *D*. *A* did 21 experiments in this series, *D* 18, and *J* 16.

Results. *A* reported 3 heats, 5 intense warmths, and 13 moderate or mild warmths; *D*, 15 heats and 3 warmths; and *J*, 2 burning heats,[15] 10 heats, and 4 warmths.

(*1*) *The quality of heat.* The *Os'* descriptions of their experiences of heat corroborated, as the following excerpts show,[16] the view that heat is a unique and further unanalyzable quality.

D: (10°-43°) "Cold at first then hot . . . a smarting pressury experience. It is not painful;" (12°-43.5°) Heat . . . a pressury smart, but withal pleasant."
J: (10°-41°) "Heat of high intensity at first . . . a pricky experience."

(*2*) *Temporal aspects.* In the experiments in which heat was aroused—the only ones we are interested in at present—all of the *Os* reported, as the following excerpts from their introspections show, qualitative and intensive changes.

A: (14°-42°) "Cold, immediately followed by hot, then by a sensation of cold again;" (12°-43°) "Cold at first . . . then, after about a second, heat which gradually declined and disappeared, and then cold again."
D: (14°-38°) "Cold at first, then hot. The intensity of the heat diminished rapidly (probably due to the adaptation of the warmth) until the experience was indifferent, then it became cool, and then in the latter part of the experiment, cold.
J: (12°-42°) "A flash of cold, followed by a mild heat which adapted to cold before the end of the period."

With the exception of pressure, which was a constant accompaniment, cold was the quality most frequently experienced first.[17] This was followed by heat, which frequently gave way in turn to cold again.[18] Exceptions to this

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[15]The temperature combination used, when *J* reported the burning heats, was 12°-42.5°C. It is very likely that the warm temperature encroached upon *J*'s thermal pain limen, for he reported burning heat both times that 42.5° was used. Too few experiments were taken in the preliminary series to do more than to establish roughly the critical point. We could not, however, if we were to complete the main experiments in the short time at our disposal, devote more time to the preliminary series.

[16]*A*, the untrained *O*, did not attempt a description of his experiences in this series.

[17]This corroborates the results obtained by Cutolo (*op. cit.*, 444), Alston (*op. cit.*, 305), and Burnett and Dallenbach (*op. cit.*, 426).

[18]The sequence, cold, heat, cold, may be due to the shorter latent period for cold (cf. A. Goldscheider, *Arch. f.* [*Anat. u.*] *Physiol.*, 1887, 468-472; and F. Kiesow and M. Ponzo, *Arch. f. d. ges. Psychol.*, 16, 1910, 376-396) and to the shorter adaptation time for warmth.

general rule occurred most frequently in *J*'s results. He reported twice as the following introspections show, that heat appeared without a prelude and changed to warmth.

J: (10°-41°) "Heat of high intensity at first, faded rapidly to a diffuse warmth;" (13°-42°) "Intense heat, followed by milder heat which adapted to warmth, and this became weaker and almost disappeared before the end of the period."

Our results clearly indicate that the temporal course of the experience must be taken into account. If it is ignored and the *O*s' reports are telescoped, *Mischempfindungen* will of course result. Such composites, however, are artifacts of the method, not items of the experience.

Series III. Burning Heat

We wished in the experiments of Series III to obtain descriptive accounts of the experiences aroused by hot stimuli.

Procedure. We brought each system of the grill, as described in Series I, to identical temperatures, which we varied in haphazard order by steps of 1° from 45°-51°C. for *A* and *D*, and from 43°-50°C. for *J*. Because of the excessive stimulation of these temperatures and the long after-effects, experiments of this series were usually postponed until near the end of the experimental hour. Except in a few cases, they were the last experiments upon a given area in a given hour. We gave *A* 15 experiments in this series, *D* 14, and *J* 16.

Results. *A* reported that 46.6% of the experiments aroused burning heat, 40% heat, and 13.3% warmth; *D*, that 78.6% aroused burning heat, 14.3% heat, and 7.1% warmth; and *J*, that 50% aroused burning heat, 43.7% heat, and 6.3% warmth.

(*1*) *Quality:* (*a*) *Warmth.* The quality of warmth was reported with the minimally hot temperatures. The *O*s merely mentioned this quality, without attempting to describe it.

A: (45°-45°) "A mild warmth, which seemed to show very little or no adaptation;" (46°-46°) "Very warm, bordering on heat."
D: (45°-45°) "Warm throughout the course of the experiment."
J: (43°-43°) "A uniform course of fairly high degree of warmth—diffuse and showing slight adaptation."

(*b*) *Heat.* The heat aroused by the hot stimuli was identical in quality, as the following excerpts show, with that aroused by the warm and cold stimuli in Series II.

A: (45°-45°) "Mild heat, gradually faded to a warmth. No pain content was experienced throughout the experience;" (46°-46°) Heat, showing no adaptation. There was an occasional addition of pain. Pain would appear, last for about a second, then disappear for a second or two, and appear again. This fluctuation seemed to be due to poor contact on the dorsal side."
D: (46°-46°) "Hot . . . a smart; a pressury painful prick."
J: (45°-45°) "A flash of heat . . . This temperature experience, so difficult to describe, seems actually to be a bright pressure-pain, localized just under the skin."

(*c*) *Burning or painful heat.* The critical temperatures for burning heat were 45°C. for *J*, and 46° for *A* and *D*. Every stimulus-temperature above these values aroused for our *O*s experiences of heat, which were, as the following excerpts from the introspections show, predominantly painful.

A: (47°-47°) "Burning heat, no adaptation. Pain and heat content about equal;" (47°-47°) "Painful burning heat. There was a very slight adaptation of the heat content, but none of the pain content;" (51°-51°) "Burning heat. Impossible, because so painful, to hold arm on grill for 10 seconds."

D: (46°-46°) "In the burning hot experience, pain is the most prominent aspect. Quality lies on the pressure-pain continuum, between prick and pain, and nearer to pain than prick;" (47°-47°) "Hot . . . a pricky stingy pressury pain;" (48°-48°) "Hot at first, then burning heat. . . . The difference between hot and burning hot is the prominence of the pain-reminder." (51°-51°) "Hot at first, then burning hot; very painful. Reflex drawing of the arm away from the grill. Experience is too painful for good observation."

J: (47°-47°) "Burn, temperature component disappeared leaving a fairly bright pricking pain;" (48°-48°) "Burn, not as intense as some of the others. There was only a flash of heat then the burn which appeared as a dull, intense pressury-pain;" (50°-50°) "Burn . . . pain is outstanding, the temperature experience is negligible. Whatever temperature is present at first goes out of the focus of attention."

We are not prepared to say, upon the basis of the results of this series, whether burning heat is a *Mischempfindung, i.e.* a mixture or fusion of the qualities of heat and pain, or whether it is, as several of the introspections indicate, a simple quality lying in the pressure-prick-pain continuum nearer to pain than to pressure. But whether we regard it as a fusion, like a simple clang, or as a simple quality, like an orangish red, so much is certain: burning heat is predominantly a pricky, stingy, painful experience. It is also significant to note, in view of Thunberg's and Kiesow's results, that stimulus-temperatures adequate to pain arouse, in contradistinction to those inadequate to it, heat experiences that are predominantly painful.

(*2*) *Temporal aspects.* The temporal course in the experiments in which burning heat was aroused was not so varied as it was in the other experiments in this series, or as in the heat experiments of Series II. When burning heat was aroused, it usually appeared immediately and did not give way to another quality during the 10-sec. interval of the experiment. *A* reported no exceptions to this general rule. *D*, however, reported that the quality of burning heat was preceded once by warmth (stimulus-temperature 51°) and twice by heat (stimulus-temperature 47° and 51° respectively), and twice that burning heat "just before the end of the experiment changed to warmth." *J* reported two

preludes: "heat" (stimulus-temperature 49°) and "a flash of cold" (stimulus-temperature 47°), but no qualitative shifts after burning heat had appeared.

While the qualitative changes in time were not as pronounced in these experiments as in the heat experiments of Series II—probably because of the greater insistence of the quality of burning heat—they nevertheless emphasize once more the necessity of taking the temporal course of the experience into account.

Series IV. Synthetic Burning Heat

We attempted, in the experiments of Series IV, to synthesize burning heat from stimuli that separately are adequate only to warmth, cold, or pain.

Apparatus and procedure. We varied the temperature of the cold system between 9°-20° for all the *O*s, and of the warm system between 40°-44° for *A* and *D*, and between 38°-42° for *J*. We aroused pain electrically. We used the two grill systems, which were made of copper tubing and isolated from one another by asbestos, as electrodes. We attached an electric wire to the binding post soldered to each system. When *O* placed his arm upon the grill he completed the circuit (if the current was turned on) and received, in the part of the arm simultaneously stimulated by warmth and cold, numerous electrical 'shocks' which were described as tingling, stingy, pricky, or painful. We tried the faradic current, but abandoned it, because of the difficulties it involved, in favor of the 110 v., 60 cycle, alternating current drawn from the University lighting line. We controlled the strength of the current, which was varied, in haphazard order, between 10-15 v., by means of a rheostat and voltmeter. *A* did 48 of these synthetic experiments, *D* 40, and *J* 36.

Results. We obtained, as the following introspections show, reports of painful or burning heat from all the *O*s.

A: (10°-43°-10 v.) "Painful heat of moderate intensity;" (11°-44°-10 v.) "Cold at first, then suddenly replaced by painful burning heat which remained constant throughout the experience;" (17°-40°-11 v.) "Cold, jumped to a painful burning heat which declined gradually to a mild warm."

D: (16°-43.5°-10 v.) "Burn of weak intensity, persisted throughout the course of the experiment;" (16°-44°-11 v.) "Burning painful heat. Either the experience is from a truly hot stimulus or else it is an excellent reproduction, as I cannot tell which it is;" (11°-42°-12 v.) "Burning hot at first, then adapted to hot, to warmth, and at the end of the experiment became cold;" (15°-42.5°-13 v.) "Burning hot at first, then warm, and then, near end of the experimental interval, burning hot again. The quality of stingy burning heat very clear."

J: (16°-42°-10 v.) "Burn at first—continued with slight adaptation to the end;" (20°-40°-11 v.) "First a neutral pressure, then a mild burn;" (14°-42°-12 v.) "Definitely burning heat—mild and diffuse."

A reported 'burning' in 12.5% of the experiments in this series, *D* in 85%, and *J* in 33.3%. *A* and *J* reported fewer cases

in this series than in Series III (normal hot stimuli), where they reported 'burning heat' in 46.6% and 50% respectively of the experiments. *D*, on the contrary, reported more cases of 'burning heat'—85% as compared to 79%—in the present series. *A*'s and *J*'s results were in accordance with our expectations, *D*'s exceeded them.

The difference between the results of *D*, on the one hand, and *A* and *J*, on the other, may be due to (1) 'set' *D* knew the object and plan of the experiment, and he may unconsciously have been biased in his reports in the direction of burning heat (*D* admits this possibility, but he is certain that he had not anticipated a particular result, and that the qualities he reported were matters of indifference to him); to (2) differences in experience and training. *D* was highly trained in cutaneous observation in general and in the introspection of heat in particular; J, though an experienced *O*, had never before observed in a cutaneous experiment; and *A* was a novice; to (3) differences in the effectiveness of the stimuli—particularly the electrical stimuli. The cutaneous stimuli were, as the experiments of Series V indicate, more effective for *D* than for either *A* or *J*, possibly because of differences in the amount of pressure of the arm upon the grill or in the amount of perspiration which bathed the arm.[19]

The lower intensities of the electrical current were more effective for all the *O*s than the higher in producing synthetically the experience of burning heat. All the 'burning heats' reported by *A* and *J* occurred when the lower intensities were used. The 'pain' experiences at the high intensities were, moreover, as the following reports show, very different in character from those aroused by the normal stimulus of burning heat.

D: (20°43°-14 v.) "Burning hot . . . The 'stings' are too coarse to duplicate the experiences of a stimulus of 50°. These 'stings' are separate, individual; the stings in the real burning heats are indistinguishable. The 'burning hot' of this experiment was like the real only for a moment." (15°-43°-15 v.) "Burning heat at first blush, then nothing but the pricky painful stings of the electrical current. The 'stings' of the current are too coarse to duplicate, beyond the first moment of the experiment, the experience aroused by a burning hot stimulus."

The difference between the 'stinging' pains aroused in these experiments and those aroused by the normal stimulus is more clearly brought out by the following report given by *D* after being stimulated by a temperature of 51°C.

[19] *A* and *J* each weighed about 130 lbs., *D* about 210; the room temperature varied between 30°-32°C.

D: (51°-51°) "Warm for an instant, then burning heat. The 'pain' or 'sting' is uniformly spread over the stimulated area; it is not punctiform as in the last experiments [with strong electrical stimuli], but is uniform like a smart. In order to reproduce the experience of burning heat synthetically, one must have a 'pain' that is areal and not punctiform."

While the results of this series of experiments clearly show, in confirmation of the analysis of Series III, that burning heat may be aroused by stimuli that singly are adequate only to warmth, cold, or pain (areal sting), they tell us nothing regarding the ultimate nature of the experience.[20] The question, whether burning heat is a *Mischempfindung* or a unique quality that lies in the pressure-prick-pain continuum, still remains an open one.

Series V. Control Experiments

After completing Series II, III, and IV, we gave the *Os* the experiments of Series V to learn, for purposes of comparison, the separate effect of the electrical current.

Procedure. We brought the temperature of each system of the grill to neutrality (33.5°-34.5°), and used, in haphazard order, the strengths of current (10 v.-13 v.) that gave the best 'burning heats' in Series IV. Under these conditions *A* observed in 21 experiments, *D* in 18, and *J* in 24.

Results. We obtained nothing that resembled heat, burn, or burning heat in the slightest degree, or that could be confused or mistaken for it. The *Os* described their experiences as pressury pricks which were mild, intense, biting, and sometimes tingling. The 'pricks' were uniformly reported as punctiform, sometimes evenly distributed over the stimulated surface, at other times scattered in an irregular mosaic.

Corollary Results

We report, as a matter of record, the following results which confirm those obtained by previous investigators. Though the *Os* were not directed in the instructions to do so, they frequently reported the spatial characteristics of their experiences, and the after-sensations aroused.

(*1*) *Spatial characteristics.* The *Os* reported, in every series of experiments, that several qualities were at times experienced together, not as fusions, but as locally distinct experiences which ran their courses separately. One quality might be experienced at one part of the stimulated area, another

[20] In Series III, *D* and *J* gave reports indicating that burning heat is a unique and unanalyzable experience. We hoped to gain further evidence in this series but were disappointed as *J* did not touch upon the point, and *D* referred to it only once. We give the instance. *D:* (10°-43°-12 v.) "Cold at first blush, then burning hot, and then hot without the burn. Burning hot is hot plus areal sting; it is a perception, *not* a simple quality." Whether this holds only for experiences synthetically aroused or applied to all burning heats, we cannot say. It is significant however, that *D* did not limit or qualify his statement, and that he gave no evidence of appreciating that the experience was synthetically aroused. This introspection, moreover, was given after those mentioned in Series III—in the third from the last experiment given in the regular series.

quality at another part, and still a third quality at a third part. For example: D, (10°-43°-12 v.) "Cold along the right edge, warm along the left, and burning hot in the middle."

The experiences were localized on and in the arm. They were described as 'surfacy,' 'cutaneous,' 'on the skin,' or as 'deep,' 'subcutaneous,' 'under the skin.' We are unable, principally because too few experiences were localized, to draw any correlation between the quality of experience and the localization. There is some indication, however, in the results of *J*—who reported this aspect more frequently than either of the other *O*s—that 'heat' is more superficial than 'burning heat,' that burning heat involves more tissues and tends to be localized deeper in the skin than heat.

(*2*) *After-experiences.* Very pronounced after-experiences were reported by all the *O*s. In Series II (synthetic heat), *A* reported "wetness," "prick," "cold," "warmth;" and *D* reported "wetness." *J* did not mention any in the experiments of this series. In series III (burning heat), *A* reported "warm glow," "areal sting," "pain;" *D* reported "smart," "burning pain," "smarting pain;" and *J* reported "burn," "burning pain." In Series IV (synthetic burning heat), *A* reported "wetness," "cold," "sting," "stinging pain;" *D* reported "wetness," "smart," "burn," "smarting stinging glow;" and *J* reported "burning sting."

The after-experiences aroused in Series II are qualitatively different and, in general, less intense than in Series III. Those aroused in Series IV are intermediate both in quality and in intensity.

Summary

The results of this study may be summarized as follows.

(1) The limen for heat—*i.e.* the point at which paradoxical cold is aroused—varies for different *O*s.

(2) The experiences of heat, aroused by stimuli that are separately adequate only to warm or cold, is a unique quality that lies in the pressure-prick-pain continuum, closer to pressure than to pain.

(3) Burning heat is predominantly a pricky, stingy, painful experience that may be aroused synthetically by stimuli that singly are adequate only to warmth, cold, or pain (areal sting).

(4) The ultimate nature of the experience of burning heat, *i.e.* whether it is a *Mischempfindung* or a unique quality that lies in the pressure-prick-pain continuum nearer to pain than to pressure, is still undecided.

A COMPARATIVE STUDY OF VOLUNTARY AND HYPNOTIC CATALEPSY[1]

By Griffith W. Williams, University of Wisconsin

Introduction

There are two rather widely quoted studies of the effect of catalepsy on the movements and oscillations of the extended arm. One of these is by Rieger who made a comparative study of normal and trance catalepsy with one other person and himself as subjects.[2] He tried to record the movements of the arm by means of a pencil held in the fingers and placed directly in contact with the paper on a revolving drum. He found, however, that the arm made horizontal as well as vertical movements. This, together with the fact that adequate pressure to secure a tracing would have obscured some of its essential features, led him to abandon this method. He then tried the ingenious method of holding a pin between the fingers and having its shadow cast on the revolving paper. A record was made by having the experimenter trace the course of this shadow with an inked brush. He claims that this gives satisfactory results and several of the tracings are reproduced in the reports of his work.

Rieger found that the arm of the hypnotic cataleptic subject dropped 18 cm. in one hour. He does not mention the rate of the dropping of the arm in voluntary rigidity, but his conclusion can be inferred from his statement that it is impossible to hold the arm in the same position for 2 min. in the normal state in spite of the subject's efforts to do so. During the trance a considerable increase in the oscillation of the extended arm was also found at the end of an hour. No feelings of fatigue were reported after experimentation in the trance. He noticed that breathing affected the movements of the arm so that the latter became somewhat rhythmical. He also claimed that the arm-movements were affected by the heartbeat.

A source of error is introduced into the results of Rieger by the fact that the instructions in the trance and normal states were not identical. In the normal state, the subjects were told to hold the arm rigidly in a horizontal position. In the cataleptic state, the arm was lifted into the horizontal position by the experimenter and left there. Such lack of comparability between trance and normal conditions is an error which is by no means unknown in hypnotic experimentation.

Charcot has also investigated this problem.[3] There is no description of his apparatus in the references given here, but its general characteristics can be

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[1]From the Psychological Laboratory, University of Wisconsin. This is the fifth of a series of Wisconsin studies on hypnotic phenomena. The writer wishes to acknowledge his indebtedness for the aid that he received from Professor Clark L. Hull.

[2]C. Rieger, *Der Hypnotismus*, 1884, 18 f.

[3]Reported by A. Binet and C. Féré, *Animal Magnetism*, 1888, 120 f.; and also by J. Crocq, *L'hypnotisme scientifique*, 1900, 26 f.

inferred from a drawing published by Binet and Féré.[4] This drawing shows a Marey's tambour attached to the back of the hand with the arm held in an extended position on the level of the shoulder. A weight is attached to, and rests upon, the rubber dam of the tambour so that a quick movement of the hand will cause a disturbance of the air-pressure within the cup owing to the inertia

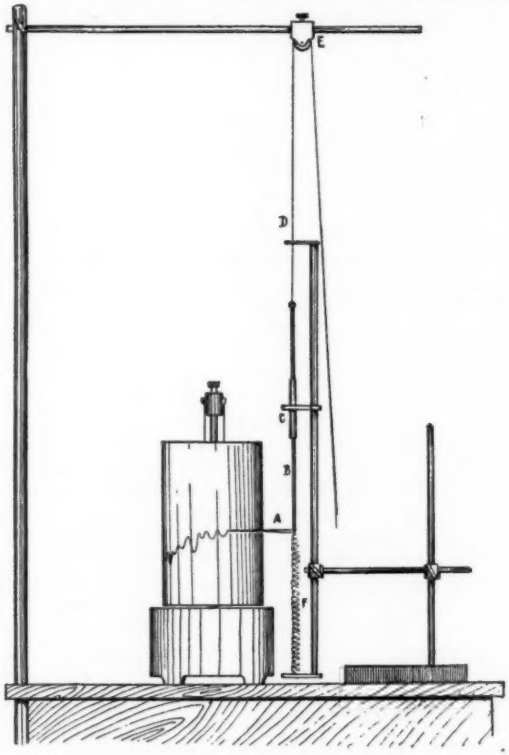

FIG. 1. DIAGRAM OF THE APPARATUS
(See text for description)

of the weight. The tambour on the hand is connected with another tambour and recording lever by means of rubber tubing. The lever on the latter tambour makes a tracing on the drum whenever there is a disturbance of the air pressure within the first tambour. This apparatus will record any rapid movement of the hand but will not record the gradual falling of the arm or any slow movements.

[4] *Op.* cit., 121.

FIG. 2. TRACINGS OF *Re*

A is a normal and B a trance record. Each record shows a tracing of breathing at the top, then a tracing of the arm-movements, a signal-line, and a time-line. The time-lines are broken at intervals of 5-sec.

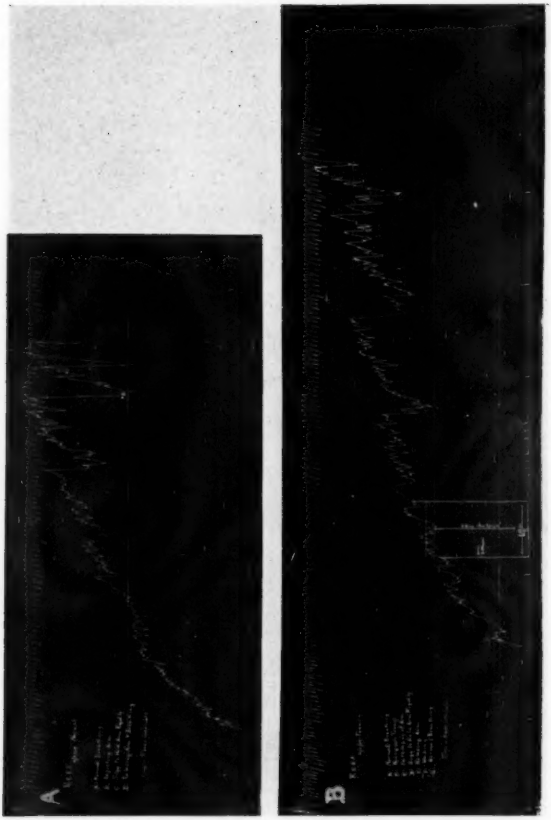

FIG. 3. TRACINGS OF *T*

A is a normal and B a trance record. Each record shows a tracing of breathing at the top, then a tracing of the arm-movements, a signal-line, and a time-line. The time-line is broken at intervals of 5-sec. in A, and of 10-sec. in B

Charcot used only one hypnotic subject, a hystero-epileptic patient. Records obtained from this subject show no trace of oscillation in the cataleptic state. In the normal state, when another subject "attempts to maintain the cataleptic attitude," the records show that oscillation soon begins and increases rather rapidly in amount. Charcot claims that the arm drops slowly and evenly in the cataleptic state although it seems evident that no objective record of this could be made with the apparatus shown in the drawing. He reports no essential difference in the length of the periods during which the arm is held up.

Binet and Féré[5] and Moll[6] quote these results as a satisfactory test for simulation. Bramwell, however, wisely adds the caution that "as most, if not all, of the muscular feats of hypnosis could be reproduced by trained unhypnotized persons, the phenomena are only worthy of note when it can be clearly proved that they are beyond the hypnotic subject's waking powers."[7]

Statement of Problem

The present experiment is an attempt to make a comparative study of the involuntary movements of striped muscle in hypnotic and voluntary catalepsy. The experimental series was so arranged that the subjects themselves served as their own controls and all conditions, except that of being in the trance or normal state, were kept constant.

Method and Procedure

The arm was used because of its convenience in experimentation. The movements studied would thus be the descent of the arm from an extended horizontal position toward a vertical position, and the oscillation of the arm during this period. Inspection of the movements of the arm reveals the fact that both horizontal and vertical oscillations occur, but in the present experiment only the vertical are studied as it was felt that the horizontal ones would show the same general characteristics.

Apparatus and technique. A tracer was constructed in which a fine moveable stylus *A* (Fig. I) is attached to a rod *B* which runs through a metal sleeve *C*. A thread *D* is attached to the rod and runs over a pulley *E*, thence down to the wrist of *S* where it is attached to a leather cuff. The contact of the pointer *A* with the smoked paper is held at a constant but very light pressure by a hair-spring *F*. The downward movement of the arm is recorded as an upward tracing on the paper.

The tracings were made on smoked paper 10 inches wide. The horizontal length of the record is determined by the rate of the falling of the arm as the stylus moves off the top of the paper when the arm has fallen a certain distance. Some of the records of two of the *S*s were terminated by the experimenter when the individuals showed such evidence of acute pain as profuse perspiration. These latter were the longest records made. A pneumograph tracing was taken simultaneously in order to discover any relation that might exist between

[5] *Op. cit.*, 133.
[6] A. Moll, *Hypnotism*, 4th ed., 1913, 208.
[7] J. M. Bramwell, *Hypnotism*, 3rd ed., 1921, 80.

breathing and the arm-movements. The *S*s sat in a chair so that the arm was free to move in any direction. The right arm was used for all *S*s except one who had received an injury to her right arm in childhood.

The *S* were put into the trance by having them look into one of *E*'s eyes. Verbal suggestions of being in a deep trance were also used when *S*'s eyes had closed. Cataleptic rigidity of the arm was induced by verbal suggestions that the arm was becoming stiff and rigid. The arm was simultaneously lifted to the level of the shoulder by *E*, and the thread was then attached to the cuff

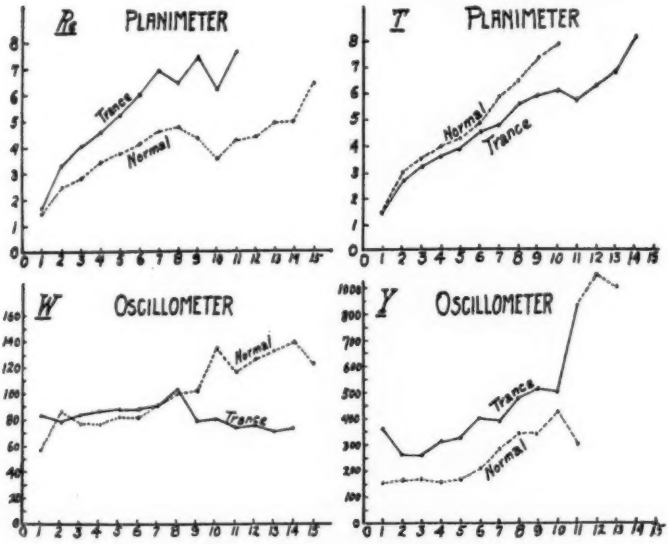

FIG. 4. TYPICAL GRAPHS OF *Re*, *T*, *W*, AND *Y*

Re and *T* show opposite tendencies in the rate of dropping the arm. *W*'s curve resembles closely the composite curve of the group of *S*s; *Y*'s illustrates an extreme case in which there is more oscillation in the trance than in the normal state.

about *S*'s wrist. In the normal state the arm was similarly lifted, but without the repeated suggestions of catalepsy. A voluntary catalepsy was induced by giving the single instruction, "Hold your arm there rigidly." This instruction was also given after the arm had been lifted in the trance cataleptic state. This was the only instruction given, so that conditions were identical when the arm had been made cataleptic and lifted in the trance state and had been similarly lifted by *E* with voluntary catalepsy in the normal state.

Every *S* served at a regular time of the day and at regular intervals. The time of day, however, varied for the different individuals.

A control series was secured by alternating the experimental series as follows: N(ormal), T(rance), T, N, T, N, N, T. At least two practice periods were given before the eight records summarized in the tables were made. This arrangement of the experimental periods provides both a normal control series and a means of equalizing practice effects.

This apparatus and technique provide a basis for two measurements of the effect of catalepsy on the movements of the arm: (1) a curve of the rate of fall-

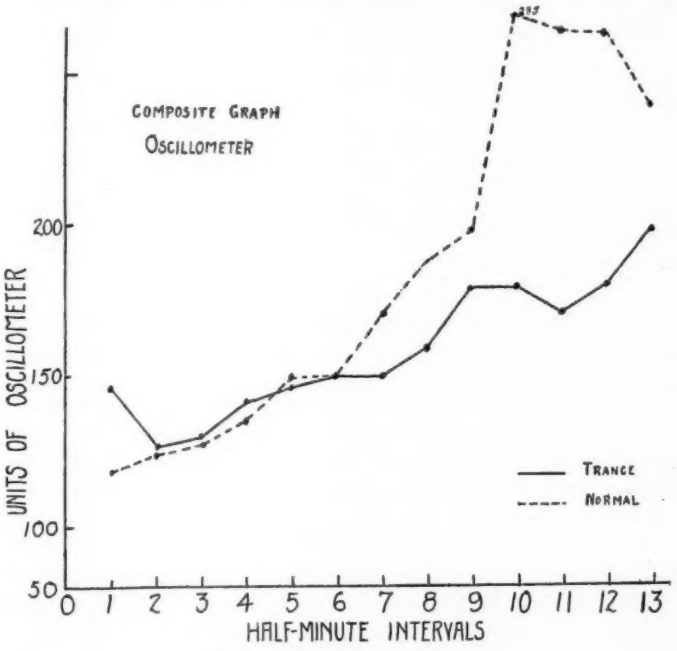

FIG. 5. COMPOSITE GRAPHS

Plotted from the averages shown in Table IV of the entire group of *S*s

ing of the arm; and (2) a curve of the amount of vertical oscillation of the arm. Four illustrative records are reproduced in Figs. 2 and 3.

The curve of the falling of the arm was secured from measurements of the tracings made with a planimeter. The records were first divided into 1/2-min. intervals. Verticals were then drawn at these points so that together with the tracing at the top and the time-line at the base, they formed irregular polygons. When the area of one of these polygons is divided by the length of the base line, the average height of the line traced by the falling arm is secured. An illus-

trative example of such a measurement is outlined in Fig. 2, B. In this polygon are indicated (1) the length of the base line, *e.g.* 2.12 in., (2) the area of the polygon, *e.g.* 8.29 sq. in., and (3) the average height of the line of tracing, *e.g.* 3.91 in. This latter is indicated by the height of the horizontal dotted line. Similar measurements were made for all the 1/2-min. intervals in the 64 records.

The records were also measured by Hull's linear oscillometer.[8] By means of this instrument it is possible to summate the vertical oscillatory movements made by the arm. The line of the record is traced with the pointer of this instrument. The upward movements are summated on one dial and downward movements on the other. The units on the dials are approximately 1/40th of an inch. The interval for these measurements was also a half-minute. One of these outlined in Fig. 3B.

Subjects. Eight *Ss* (2 men and 6 women, and all students) were used. Six of them were somnambules; such phenomena as automatic writing and post-hypnotic amnesia could be produced in them. The other two were not as susceptible; they could be put into a light trance in which catalepsy was easily induced, but not in a state that induced posthypnotic amnesia. None of the *Ss* knew the object of the experiment. They also worked without knowledge of their results.

Results

An illustration of how the data were treated is shown in the accompanying tables. Table I shows the detailed planimeter readings for two records. These records are reproduced in Fig. 2 and one of the intervals measured has been outlined. With the 1/2-min. interval on the time-line as base, the area enclosed by this base, two verticals, and the tracing, was measured. The average height of the tracing was then found for each unit measured. These averages are also shown in the table.

When the records of a *S* had been measured and the heights of the tracings ascertained, the totals of the average heights of the tracings of the 4 trance and 4 normal records, respectively, were secured. An average of these was secured as shown in Table III and individual graphs were plotted from these averages. Two typical graphs are reproduced in Fig. 4. *Re*'s graph is typical of those *Ss* who show a more rapid falling of the arm in the trance than in the waking state; *T*'s graph is typical of those *Ss* whose arm falls more rapidly in the normal state.

There is no satisfactory way of separately measuring the amount of movement due specifically to the falling of the arm as distinguished from its downward oscillation. In the measure-

[8] C. L. Hull, An instrument for summating the oscillations of a line, *J. Exper. Psychol.*, 12, 1929, 359-861.

TABLE I

PLANIMETER MEASUREMENTS OF THE TRACINGS OF *Re* THAT ARE REPRODUCED IN FIG. 2

Record	Type	Variable	Half-minute intervals										
			1	2	3	4	5	6	7	8	9	10	11
A	N	area	3.55	5.80	6.50	8.22	8.16	10.23	10.70	12.11	9.78		
		length	1.50	1.50	1.50	1.50	1.44	1.50	1.60	1.60	1.56		
		av. heights	2.37	3.87	4.33	5.48	5.67	6.82	6.69	7.57	6.27		
B	T	area	2.23	5.14	8.29	8.60	8.87	9.18	11.09	8.93	11.00	11.13	6.29
		length	1.40	1.80	*2.12	1.80	1.74	1.80	1.80	1.80	1.74	1.78	0.82
		av. heights	1.59	2.85	*3.91	4.78	5.10	5.10	6.16	5.13	6.25	6.25	7.67

*This interval is outlined in Fig. 2B.

TABLE II

OSCILLOMETER MEASUREMENTS OF THE TRACINGS OF *T* THAT ARE REPRODUCED IN FIG. 3

Record	Type	Movement	Half-minute intervals									
			1	2	3	4	5	6	7	8	9	10
A	N	downward	107	96	53	31	60	65	72	60	51	
		upward	45	47	22	50	27	38	27	22	28	
B	T	downward	146	92	93	68	67	68	62	63	70	63
		upward	60	*51	52	71	47	42	42	46	53	34

*This interval is outlined in Fig. 3B.

ments shown, this movement is included in the downward amount of oscillation. As this is constant for all records, however, it does not invalidate the comparative conclusions of this study.

The data obtained from measuring the records with the oscillometer were treated in a somewhat similar way. The upward and downward oscillations for every 1/2-min. interval were measured and recorded as shown in Table II. The records from which the measurements in this table were made are shown in Fig. 3, A and B. The upward and downward oscillations were added together for each interval, and the totals averaged for all normal and all trance records respectively. These averages are shown in Table IV. Individual graphs were then plotted from these averages. Two graphs of the relative oscillations in the normal and trance states are reproduced in Fig. 4. *W* shows an approximately equal amount of oscillation in both states to the end of the fourth minute. The graph of *Y* is included to show an extreme case where there is more oscillation in the trance than in the normal state.

Table III is a composite table including the average heights of the tracings of all *S*s determined from planimeter measurements. Separate averages are given for the normal and trance records respectively. It thus provides a summary of all the measurements made with the planimeter. A final composite average for the normal and trance records respectively is given in the last two rows of this table. The oscillometer readings are summarized in the same way in Table IV. The graph (Fig. 5) of these final oscillometer averages is included.

To ascertain whether the differences between the composite averages of the trance and normal records were true differences or merely due to chance, it was necessary to compute the P.E. of the difference between these averages. The computations, therefore, were made for the averages of the ninth, tenth, and eleventh 1/2-min. intervals and the following values obtained: 52.96, 73.43, and 103.56. When these values were substituted in the formula: D/PE_{diff}, in which D is the difference, values of 0.39, 0.33, and 0.28 were obtained respectively. The chances in 100 that the true difference in each of these cases is greater than zero are 60, 59, and 58, respectively. These averages are thus unreliable statistically, and at best, merely indicate a possibility. In general, it means that if this experiment were repeated with an infinite number of *S*, the chances are 6 out of ten that the difference would be

TABLE III

SHOWING FOR EVERY *S* AND FOR THE GROUP AS A WHOLE THE AVERAGE HEIGHT OF THEIR TRANCE AND NORMAL TRACINGS

The averages for every *S* were obtained from 4 records each, those for the group from 32 records each.

		Half-minute Intervals																								
S	Type	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
C	T	1.32	2.16	2.84	3.99	5.49	6.94	8.61																		
	N	1.19	1.48	2.29	3.38	4.81	6.55	7.4	57.97	8.72																
Ra	T	1.37	2.44	3.60	4.52	5.33	6.25	6.96	8.00	8.72																
	N	1.61	2.45	3.70	4.61	5.74	6.11	6.72	7.33	7.92	8.14	7.60	8.10													
Y	T	1.51	2.78	3.29	3.66	3.80	4.07	5.09	4.55	5.25	5.12	3.66	3.79	5.47												
	N	1.30	1.72	2.37	2.43	3.81	3.16	4.48	4.19	3.97	5.14	6.99	5.48													
T	T	1.48	2.63	3.31	3.63	3.96	4.53	4.80	5.60	5.92	6.13	5.76	6.26	6.84	8.15											
	N	1.63	2.97	3.55	4.00	4.30	4.87	5.86	6.50	7.34	7.85															
B	T	0.97	1.75	2.03	2.42	2.69	3.01	3.55	4.14	4.56	3.99	4.24	5.07	6.65	7.87											
	N	1.03	1.43	1.99	2.50	3.32	3.69	4.20	5.23	5.68	6.44	6.89	7.06	8.00												
Rs	T	1.72	3.55	4.06	4.60	5.28	6.07	6.95	6.51	7.43	6.25	7.67														
	N	1.52	2.43	2.81	3.45	3.82	4.16	4.63	4.77	4.40	3.61	4.34	4.46	4.98	5.04	6.49										
F	T	1.20	1.31	1.33	1.58	1.82	2.03	2.26	2.24	3.03	3.84	4.08	4.78	4.73	3.66	4.09	4.44	6.58	7.25	7.43	6.67	7.73				
	N	0.82	1.32	1.73	2.10	2.77	3.48	4.07	4.53	5.61	6.34	5.44	5.93	5.08	6.31											
W	T	1.15	1.62	2.13	2.64	3.36	4.08	4.91	5.76	6.11	6.61	7.34	7.67	8.71	8.44	8.36										
	N	0.87	0.92	1.02	1.29	1.16	1.38	1.48	1.78	2.20	2.67	2.90	3.20	3.55	3.90	4.19	4.62	4.78	5.16	4.69	4.72	5.53	5.57	5.63	5.79	5.77
Composite	T	1.33	2.25	2.81	3.78	3.97	4.55	5.29	5.23	5.82	5.21	5.44	5.87	6.84	7.10	5.51	4.44	6.58	7.52	7.43	6.67	7.73				
	N	1.25	1.83	2.43	2.97	3.71	4.17	4.63	5.04	5.52	5.66	5.09	4.80	4.65	4.49	3.88	4.62	4.78	5.16	4.69	4.72	5.53	5.57	5.63	5.79	5.77

in the same direction, but 4 out of ten that they would be in the opposite direction. About the weakest reliability that is accepted in experimental science is 0.95, *i.e.* 95 chances out of a hundred of its being true and only 5 in a hundred of its being false.

Comments and Conclusions

It has been generally assumed that the hypnotic trance tends to decrease individual differences, but this experiment illustrates to a marked degree the retention of differences in performance by *S* in the trance. The range of performance represented by the 8 *S*s in both of the measurements used is very striking and the statistical reliability of any average made from such measurements will be small. A similar wide range of variability in muscular reactions during hypnosis was found in work previously reported by the writer.[9]

The *S*s reported an absence of feelings of pain in the arm after the taking of the trance records. This was not due to any conscious suggestion given by the experimenter. Rieger[10] reports the later occurrence of fatigue, and Binet and Féré[11] its non-appearance in the trance. The failure to report any feelings of pain in this experiment is analogous, if not identical with these observations. Acute pain was reported during the making of the normal records.

No characteristic difference appears in the gross average length of time during which the arm was held up or in the curve of the falling of the arm. Table III shows that 4 *S* held their arms up longer in the trance and four in the normal state. This seems to confirm Moll's conclusion that "a cataleptic person cannot hold his arms up longer than an imposter."[12] It raises a question, however, concerning the interpretation of the reported absence of feelings of pain. Normally, it would be expected that the arm would be held up longer when there are no feelings of pain. It is possible that there are other factors functioning in the trance which would counterbalance the effects of this analgesia, but it is also possible that the analgesia may merely be an inhibition of verbalized reports of feelings of pain and that otherwise the physiological characteristics of pain are just as acute in the trance as in

[9] G. W. Williams, The effect of hypnosis on muscular fatigue, *J. Abn. Psychol. & Soc. Psychol.*, 24, 1929, 318–329.
[10] *Op. cit.*, 59, 67.
[11] *Op. cit.*, 133.
[12] *Op. cit.*, 208.

TABLE IV

SHOWING FOR EVERY *S* AND FOR THE GROUP AS A WHOLE THE AVERAGE AMOUNT OF OSCILLATION AS MEASURED IN UNITS OF THE OSCILLOMETER

The averages for every *S* were obtained from 4 records each, those for the group from 32 records each.

		Half-minute Intervals																							
S	Type	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
C	T	168	150	154	142	156	164																			
	N	200	217	253	301	318	273	331	314																	
Ra	T	100	74	74	62	84	76	84	73	69																
	N	103	87	91	102	86	75	66	82	90	102	63	109													
Y	T	357	263	259	307	329	398	390	476	512	502	830	1187	981												
	N	154	161	163	158	166	208	281	344	345	428	305														
T	T	156	129	133	145	115	97	110	119	105	133	118	90	124												
	N	140	153	119	137	126	174	149	139	146																
B	T	110	82	94	97	99	99	114	117	125	130	126	186	222												
	N	89	94	99	96	119	101	114	108	88	125	81	88													
Re	T	173	227	227	263	286	251	277	266	302	570															
	N	185	190	225	211	274	253	323	491	503	576	641	794	687	933											
F	T	100	95	96	109	96	111	114	133	146	146	197	144	209	207	282	243	176	154	210	321					
	N	102	83	72	77	103	116	124	164	196	272	236	181	285	342											
W	T	83	79	83	86	87	87	90	103	78	80	74	75	71	73											
	N	56	85	77	77	82	82	90	99	101	133	116	125	132	138	122	146	134	136	146	116	176	122	132	178	162
Composite	T	156	137	140	151	156	160	160	169	189	189	181	190	208	140	282	243	176	154	210	321					
	N	128	134	137	145	159	160	180	197	208	285	273	274	250	305	122	146	134	136	146	116	176	122	132	178	162

the normal state. These facts suggest a problem that would probably yield fruitful results for investigation.

A curve plotted from the composite averages shown in Table III closely resembles that of *T* (Fig 4) in general characteristics except that the composite curve shows (1) less negative acceleration, and (2) the cataleptic arm falls slightly faster than the normal state. The obtained differences in the averages from which the composite curve was plotted (Table III) were so small that it seemed unnecessary to make any further statistical computations. On the basis of the work done in the present experiment the curve of the falling of the arm may then be regarded as having a very slight negative acceleration and as essentially like the individual curve to which reference has been made.

The curves of oscillation, however, seem to show a clearer distinction between the trance and the normal performance. Five *S*s show less oscillation in the trance, 2 show more, and one shows an equal amount of oscillation in both states. A composite graph shows an equal amount of oscillation up to the end of the sixth 1/2-min. interval and then a marked increase in oscillation in the normal state as compared with the trance. This difference, however, was found upon investigation to be statistically unreliable. Curves were not plotted beyond the thirteenth 1/2-min. interval because the number of units included in the averages is small, as may be seen from an examination of Table IV. The general characteristics of the curve of the amount of oscillation contrast with those of the curve of the falling of the arm. The curves of both the normal and trance oscillation show positive acceleration. This is quite marked in the case of the normal oscillation.

The statistical reliability of the differences of the averages, however, indicates that too much emphasis should not be placed on the shape of the curve. It is probable that much of the present theory of hypnosis would be modified if adequate statistical treatment were applied to the results of experimentation. The convincing appearance of the curve as contrasted with its statistical unreliability raises the question of the value of the work of Charcot, Rieger, and others, when they report experiments on only one subject. Rieger, Binet and Féré, and Moll report oscillation in the normal state as contrasted with no oscillation in the trance. The results of the present experiment merely indicate that there

is a tendency for more oscillation to occur in the normal than in the trance state. The fact that the difference in amount of oscillation does not occur at the beginning of the periods does not seem to have been previously reported.

Breathing has a pronounced effect on the movements of the arm as is shown by an inspection of any of the records. Breathing introduces a rhythmical movement into the tracing made by the arm. It often occurs, *e.g.* in Fig. 2, B, that a large movement of the arm is synchronous with deeper or more uneven breathing. Rieger reports this same characteristic and it seems probable that most of the oscillations shown on the charts published by him are due to breathing and do not represent the independent oscillations of the arm at all.

The following conclusions seem to be warranted by the data presented in this study.

(1) There is a failure to report feelings of pain during the making of the trance records which is not due to any suggestion given by the experimenter. The question of the relation of feelings of pain to work accomplished was, however, not specifically studied in this experiment.

(2) The *S*s used in this experiment were unable to hold up their arms any longer in the trance than in the waking state.

(3) There is possibly an increase in the amount of oscillation in the normal state as compared with that in the trance state after the first three minutes of experimentation.

(4) The characteristic curve of the falling of the arm shows slight negative acceleration and that of the amount of oscillation a more clearly emphasized positive acceleration.

DEFINITE AND INDEFINITE PREPARATION IN THE VISUAL APPREHENSION EXPERIMENT

By Miles A. Tinker, Dorothy Roberts, and Harriet Jackson
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Most investigators, in determining the span of visual apprehension, have employed some type of tachistoscope for presenting stimuli. To maintain conditions which yield the most valid results various authorities agree that this short exposure technique must be carefully controlled. Evaluation and criticism of the tachistoscopic technique have been made by Becher, Binet, Cattell, Dearborn, Erdmann and Dodge, Dodge, Goldscheider and Müller, Hoffmann, Huey, Kutzner, Schumann, Whipple, Wundt, and Zeitler.[1] Wundt, in his discussion, formulated seven essentials of a good tachistoscope. One of these requirements is that a ready-signal must be given at an appropriate time before the exposure. The question arises what is the appropriate time-interval and how definitely must the observer be prepared in order to respond most efficiently. In reaction-time experiments a preparatory interval of 2 sec. has been found most favorable for yielding a quick response to stimulation. Also definite preparation (a definite time-interval between the ready signal and the exposure) produces faster reaction than indefinite preparation.[2]

In many tachistoscopic studies of visual apprehension the preparatory interval has been 2 sec. and there has always been definite preparation. This interval is convenient to use and it appears adequate for this kind of experimentation. Does the observer, however, need, in order to respond efficiently, as definite a preparation in the visual apprehension as in the reaction-time experiment? Our problem is to determine the relative efficiency of apprehension with definite and indefinite preparation in the visual apprehension experiment when the time of exposing stimuli is held constant.

Method. The tachistoscopic method of presentation was employed to determine the span of visual apprehension for letters in nonsense series under each of the two conditions of the experiment. Two students (*J* and *R*) and one member of the faculty (*T*) served as *Ss* in this investigation. Every *S* had had previous experience in short exposure observation.

The stimulus material consisted of 200 groups of capital letters arranged in nonsense series. The letters were about 7 mm. high and each series was printed with black india ink in a horizontal line 4.5 cm. long. The line of letters was centered on a white card 9 by 9 cm. To avoid nonsense syllables only consonants were employed in constructing the groups of letters. There were 7 letters in each series. Additional stimulus cards were prepared for the practice series.

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[1]For references and discussion see G. M. Whipple, *Manual of Mental and Physical Tests*, 1910, 222 ff., 243 f.

[2]Cf. H. Woodrow, The measurement of attention, *Psychol. Monog.*, 17, 1914, (no. 76), 16-65.

Apparatus. A Whipple disk tachistoscope was used for presenting the stimuli. An exposure interval of 120σ was employed. This interval allowed ample time for clear perception of the exposure field, was brief enough to preclude eye-movement, and was a convenient interval to use with the apparatus. The pre-exposure field carried a fixation-mark. The *Ss* sat close to the table upon which the tachistoscope was mounted. Their eyes were on a level with the exposure field and at a distance of approximately 20 in. from the material to be read.

Procedure. Every *S* observed regularly twice a week with a few exceptions when there was only one sitting per week. The experiment consisted of two parts: the preliminary series with definite preparation and the variable series including both definite and indefinite preparation. In the preliminary part of the experiment the first sitting was a practice session. Then there were 4 regular observation periods at each of which 50 series of letters were shown.

In the second part of the experiment every *S* was shown 55 stimulus series at each sitting. The first five cards constituted a warming-up exercise and were given with a definite preparation of 2 sec. (*i.e.* a known interval of 2 sec. between the ready signal and the exposure). These 5 series were not used in computations. The next five cards were also given with a definite preparation of 2 sec. For the remaining 45 series indefinite preparation was employed. The intervals used with indefinite preparation were 2, 4, 8, 12, 16, 20, 24, and 32 sec. long. The timing of these intervals was done with a stop watch. Five trials of each interval were given at a single setting. The order of the intervals with indefinite preparation was haphazard and unknown to *S*. In all there were for every *S* 20 sittings during which 100 experimental series were shown with definite preparation (2 sec.) and 100 at each interval under indefinite preparation. When the series of 200 stimulus cards had been read once the cards were shuffled and used again. This occurred once about every 2 weeks. Every day before the experimentation began the *Ss* read the following instructions (definite preparation):

"The object of this experiment is to discover how many letters can be read during a very brief given exposure. The series of 7 letters will appear at the center of the exposure window. After you have viewed the group of letters repeat the series aloud to *E*. Say 'blank' to designate the omission of a letter.

"At the ready signal fixate your vision on the cross at the center of the exposure window; 2 sec. later *E* will say 'Now' and then after 2 more sec. the stimulus will appear in the center of the region which you are fixating."

For the indefinite preparation the directions were modified informing *S* that, following the "now" signal, the stimulus might appear after any one of the several intervals cited above and that the sequence of the intervals would be varied in an irregular order.

E recorded responses as given by *S*.

Scoring. The responses were scored for the number of letters correctly reproduced in the right order. Any misplaced item was considered wrong.

Experimental Results. The preliminary series of 200 trials with definite preparation was completed in order to allow the *Ss* thoroughly to adapt themselves to the experiment and partially to stabilize the spans of visual apprehension. Table I shows the average span for every consecutive series of 50

responses and for the total of 200 presentations. *R* and *J* increased their spans slightly during this part of the experiment while *T*'s span decreased by a small amount. On the average, however, the results for the group show that the spans of visual apprehension were rather constant for the 4 sittings (column 8 of the table).

TABLE I

Span of Visual Apprehension in the Preliminary Series

The average number of items correctly read per exposure for each successive group of 50 trials and for the whole 200 trials with the standard deviations of the distributions for every *S* and for the group.

No. of Readings	*R*		*T*		*J*		All *S*s	
	Av.	σdis.	Av.	σdis.	Av.	σdis.	Av.	σdis.
1st 50	3.22	0.37	3.28	0.36	4.04	0.92	3.51	0.55
2nd 50	3.30	0.49	3.02	0.38	3.92	0.95	3.41	0.61
3rd 50	3.34	0.42	3.18	0.63	4.24	0.66	3.59	0.57
4th 50	3.42	0.56	2.94	0.38	4.32	0.54	3.56	0.49
All 200	3.32	0.70	3.11	0.67	4.13	0.89	3.52	0.75

TABLE II

The Influence of Definite and Indefinite Preparation on the Span of Visual Apprehension

The average number of items correctly read per exposure and the standard deviations of the distributions for every *S* and for the group. 100 readings for every *S* at each interval.

Preparation	Interval in Sec.	Subjects						All *S*s	
		R		*T*		*J*			
		Av.	σdis.	Av.	σdis.	Av.	σdis.	Av.	σdis.
Def.	2	3.70	0.93	3.58	0.74	4.43	1.00	3.90	0.95
Indefinite	2	3.63	0.95	3.67	0.75	4.30	1.02	3.87	0.98
	4	3.70	1.14	3.43	0.73	4.52	0.89	3.88	1.03
	8	3.87	0.96	3.46	0.75	4.45	0.95	3.93	0.99
	12	3.71	1.08	3.36	0.70	4.45	1.15	3.84	1.10
	16	3.83	1.10	3.53	0.74	4.42	1.05	3.93	1.05
	20	3.76	0.89	3.52	0.88	4.14	0.86	3.81	0.93
	24	3.86	1.09	3.44	0.71	4.38	0.94	3.89	1.05
	32	3.80	1.04	3.48	0.71	4.36	0.94	3.88	0.99
	All	3.77	1.03	3.49	0.74	4.38	0.98	3.88	1.02

Results from the second part of the experiment are given in Table II. Averages under the conditions of definite preparation are shown in the first row. These figures are to be compared with the results in the rest of the table. Examination of the averages for individual *S*s reveals some variation between definite and indefinite preparation in span of apprehension and between various intervals under indefinite preparation. For *R* the largest difference between averages is 0.24, which is found by comparing the 8 sec. with the 2 sec. interval under indefinite preparation. This difference is of doubtful

statistical significance since D/σ_{diff} yields only 1.79. When the average for R, under conditions of definite preparation, is compared with the average of all intervals with indefinite preparation, the difference is 0.07 and may be attributed to chance.

For T, however, the largest difference of 0.31 between the 2 sec. and the 12 sec. interval with indefinite preparation has high statistical significance, since $D/\sigma_{diff} = 3.04$. The next greatest difference, however, yields a D/σ_{diff} of only 2.31 which indicates comparatively low reliability for predictive purposes.

Results for J show no differences which can be assigned to other than chance factors. The largest difference of 0.22 between the 2 sec. and the 4 sec. intervals with indefinite preparation gives a D/σ_{diff} of 1.63. For both T and J the differences between averages under definite preparation and averages for all intervals with indefinite preparation are chance differences.

If the group averages for all three *S*s are considered, similar trends are discovered. The largest difference of 0.12 between the 8 sec. and the 20 sec. intervals with indefinite preparation yields $D/\sigma_{diff} = 1.54$, which indicates that the difference has little or no reliability for prediction. For these group comparisons (column 9) the average under definite preparation is almost identical with the average span for all intervals with indefinite preparation (3.90 and 3.88). The various total averages in the lower section of column 9 of the table show striking agreement. Only two of the 9 averages with indefinite preparation vary more than 0.03 of a point from the average span of 3.90 under definite preparation.

Examination of the standard deviations of the distributions will show that they are relatively constant for the various intervals with indefinite preparation, but that they are slightly greater for indefinite than for definite preparation (1.02 *vs.* 0.95). This difference is of comparatively slight statistical significance.

With definite preparation the range in span was 2 to 7 letters; for indefinite preparation 1 to 7. Ordinarily the *S*s adjusted themselves quickly and efficiently to the conditions of indefinite preparation. Occasionally, however, due to wandering of the attention, the *S*s obtained scores of only 1. On the other hand, scores of 6 were approximately twice as frequent with indefinite as with definite preparation. In general the conditions of indefinite preparation tended to foster in the *S* a relaxing and receptive attitude which seemed to aid apprehension.

The evidence presented above indicates that visual apprehension is equally efficient under either definite or indefinite preparation and that with indefinite preparation any single time interval is as effective for efficient apprehension as any other within the limits of 2 to 32 sec. Why is indefinite preparation less efficient than definite preparation in the reaction-time experiment, while the two conditions of preparation are equally effective in visual apprehension? There is an important distinction between the two experiments which may be cited as a partial explanation. The reaction-time experiment calls for an immediate motor response on perception of the stimulus and the quickness with

which the response is initiated is the measure involved. In visual apprehension the responses is a delayed reaction. Although the stimuli are directly apprehended or apprehended by re-imaging the stimulus material, the response is given at the convenience of *S*.

The results described in this report have a certain bearing on the control of tachistoscopic procedure in work with visual apprehension where the time of exposure is approximately 1/10 sec. The findings indicate primarily that no timing device is needed for controlling the length of the preparatory interval. Any interval which is convenient to work with, such as 2 sec., may be employed. A rough approximation of this interval, as determined by *E*'s counting "one, two," or by some similar device, will yield satisfactory and reliable results. It is not necessary to have *S*'s attention at a high level at the instant of the exposure, though such a condition may be highly desirable where the time of exposure is very brief, *i.e.* 5σ.

Summary and Conclusions

(1) This study was undertaken to determine the relative efficiency of apprehension with definite and indefinite preparation in the visual apprehension experiment.

(2) In a preliminary experiment the span of visual apprehension was determined for the three *S*s with a definite preparatory interval of 2 sec.

(3) In the second part of the experiment spans were obtained with a definite preparation of 2 sec. and with indefinite preparation for intervals of 2, 4, 8, 12, 16, 20, 24, and 32 sec.

(4) No reliable differences were discovered between spans with definite and spans with indefinite preparation nor between spans with preparatory intervals of different lengths under the conditions of indefinite preparation.

(5) The results warrant the conclusion that the use of either definite or indefinite preparation yields equally efficient apprehension in the visual apprehension experiment when the time of exposure is approximately 1/10 sec., the definite preparation is 2 sec., and the indefinite preparation varies between the limits of 2 and 32 sec.

TIME-LIMIT *VS.* WORK-LIMIT METHODS

By Donald G. Paterson and Miles A. Tinker, University of Minnesota

It would seem desirable to discover through experiment whether or not the time-limit method and the work-limit method of administering speed tests are equivalent and interchangeable. For the most part this question has been discussed in connection with the relative advantages and disadvantages of the group *vs.* the individual procedure in test administration. The difficulties of utilizing the work-limit method in group testing have usually been recognized with the implication that the group testing method suffers as a consequence of using the time-limit method. Theoretical considerations indicate that rate of work or speed of performance might be measured either by amount of work per unit of time (time-limit) or by the time required per unit of work (work-limit). The practical difficulties, however, of arranging test materials of equal difficulty throughout the test are held by some to be so great as to place in question results obtained by the use of the time-limit method. Such reasoning as this led Whipple to say: "There is no doubt, therefore, that the work-limit method is to be preferred to the time-limit method: it is better, in other words, that every *S* should be asked to perform the same work and to measure his efficiency in terms of elapsed time than to require every *S* to work for the same time and to measure his efficiency in 'ground covered.' But the time-limit method is compulsory in all tests of this order undertaken by groups, so that this constitutes yet another serious objection to the group-method."[1] In a footnote Whipple states that he has been able to use the work-limit method successfully with groups of reliable *S*s by the aid of a newly devised time-clock.

In a search through testing manuals we have been unable to discover any actual evidence regarding the essential identity of results obtained from the two methods under consideration. Authors seem to take it for granted that the two methods yield equivalent results and then proceed to discuss the practical advantages and disadvantages of each method in connection with a variety of testing situations.

In view of the absence of experimental data it was considered desirable to study experimentally this question through the use of a speed of reading test known to have been carefully constructed to yield two duplicate forms containing paragraphs each equal in difficulty to every other.[2] The problem is to determine the 'true' correlation (obtained r corrected for attenuation) between speed of reading scores obtained by the time-limit method and by the work-limit method. College students enrolled in elementary and advanced psycho-

[1] G. M. Whipple, *Manual of Mental and Physical Tests*, 1914, I, 8-9.

[2] J. C. Chapman and S. Cook, The principle of the single variable in a speed of reading cross-out test, *J. Educ. Res.*, 8, 1923, 389-396. Forms A and B of the Chapman-Cook Speed of Reading Tests were used.

logy classes were used as *Ss*. In all, 1090 students were tested. They were subdivided into the following four experimental groups:

Group I	Form A, Time-limit method.
(n = 185)	Form B, Work-limit method.
Group II	Form A, Work-limit method.
(n = 183)	Form B, Time-limit method.
Group III	Form A, Work-limit method.
(n = 162)	Form B, Work-limit method.
Group IV	Form A, Time-limit method.
(n = 560)	Form B, Time-limit method.

Data from Groups I and II permit the computation of coefficients of correlation between paired scores for Forms A and B. These coefficients constitute the *intercorrelations* between the two methods of testing. Groups III and IV permit the computation of similar correlations. These latter correlations are the *reliability coefficients* for the two methods of testing. Since the formula for correcting correlation coefficients attenuated by the presence of chance errors necessitates the mere division of the average intercorrelation by the average reliability coefficient our method of analysis is simple and straightforward. In practice, the correction formula employs the geometric mean. In our problem the numerator is the square root of the product of the two intercorrelations and the denominator is the square root of the product of the two reliability coefficients.[3]

Details of administration and scoring the tests followed our previous practice.[4] The time-limit employed was 1¾ min. In conducting the tests with the work-limit method a letter code was used, a different letter being placed on the blackboard every 10 sec. The students were instructed to record on their papers whatever letter happened to be on the blackboard at the instant they completed their tests. The purpose of the experiment was explained before the tests were given.

Groups I, II, and III were tested during the Spring Quarter of 1928-29. The results for Group IV were obtained in connection with another experiment (the effect of length of line on speed of reading) during the Fall Quarter of 1928-29. Evidence will be given later indicating that the results for Group IV may legitimately be included as a part of the present experiment.

Results. Table I contains all of the basic data for our comparisons. Variations in the means and standard deviations of the distributions for the different experimental groups probably reflect minor differences of sampling since classes composed of sophomores were included along with classes of advanced undergraduates and graduates.

The coefficients of variability are reported to show the effect of method upon the dispersion of reading scores. It is obvious that no definite trend exists in this respect. Thus the time-limit and the work-limit methods seem to be equally effective in the measurement of individual differences in speed of reading.

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[3]H. E. Garrett, *Statistics in Psychology and Education*, 1926, 211-213.

[4]M. A. Tinker and D. G. Paterson, Studies of typographical factors influencing speed of reading, I. Type Form, *J. Appl. Psychol.*, 12, 1928, 359-368; II. Size of type, *ibid.*, 13, 1929, 120-130; III. Length of line, *ibid.*, 13, 1929, 205-219.

TABLE I

BASIC DATA REGARDING THE TIME-LIMIT AND WORK-LIMIT METHODS OF ADMINISTERING THE CHAPMAN-COOK SPEED OF READING TESTS TO COLLEGE STUDENTS

			Mean	S.D.	C. of Var.	r	N
I.	Intercorrelations						
	Form A	Time-limit	21.12	5.02	0.24		
	Form B	Work-limit	3′8″	50″	0.27	0.87 ± 0.01	185
	Form A	Work-limit	2′50″	41″	0.24		
	Form B	Time-limit	20.30	4.29	0.21	0.84 ± 0.01	183
II.	Reliability Coefficients						
	Form A	Work-limit	2′36″	42″	0.27		
	Form B	Work-limit	2′48″	46″	0.27	0.86 ± 0.02	162
	Form A	Time-limit	19.05	4.34	0.23		
	Form B	Time-limit	17.87	3.73	0.21	*0.84 ± 0.01	560

Main interest centers in the column containing the coefficients of correlation. For Groups I, II, and III these are remarkably similar; such slight differences as exist are within the limits of the P.E. With respect to the coefficient of correlation for Group IV, it is to be noted that the standard deviations of the distributions for this group are smaller than in the preceding groups. This is because Group IV was composed of sophomores alone whereas the other three groups were composed of students with a much greater range of talent. This fact necessitates the correction of the coefficient of correlation obtained for this group ($r = +0.78 \pm 0.01$) by employing Kelley's formula[5] ($\sigma/\Sigma = \sqrt{1-R}/\sqrt{1-r}$) in order to make the obtained r comparable with the rs for the other three groups with respect to heterogeneity of subjects. The application of this formula yields a corrected r of $+0.84 \pm 0.01$.[6]

The similarity in magnitude of the two intercorrelations (+0.87 and +0.84 for Groups I and II) and the two reliability coefficients (+0.86 and +0.84 for Groups III and IV) indicates quite clearly that the two testing methods agree with each other as closely as each agrees with itself. Thus application of the correction for attenuation formula $r_{TW} = \sqrt{r_{T_1W_1} \cdot r_{W_1T_2}} / \sqrt{r_{T_1T_2} \cdot r_{W_1W_2}}$ results in a 'true' correlation of +1.00.[7] In other words the time-limit method and the work-limit method produce speed of reading scores which are perfectly correlated within the limits of the reliability of each. Hence, Whipple's strong

*r of $+0.84 \pm 0.01$ was derived from an obtained r of $+0.78 \pm 0.01$. See text for explanation.

[5]T. L. Kelley, The reliability of test scores, *J. Educ. Res.*, 5, 1921, 370-379.

[6]The use of a reliability coefficient for the time-limit method obtained when Form B differed from Form A in typographical arrangement might be questioned. Limitation in number of available subjects forced us to follow this procedure. However, the consistency with which Form A correlated with Form B in a series of eleven experimental groups (80 students in each group), in some of which Form B differed in type size, and in others in length of line, suggests that the reliability of the time-limit method as *method* is being revealed. These eleven correlations ranged from +0.70 to +0.85; the average was +0.80.

[7]H. E. Garrett, *op. cit.*, 211-213. Subscripts used above: T = Time-limit method, W = Work-limit method, 1 and 2 refer to the two forms of the test.

preference for the work-limit method fails to receive any objective substantiation in this experiment. If these results should be verified by other experiments in which various types of speed tests are utilized and administered either by the individual or by the group method of testing, it would follow that the time-limit method and the work-limit method are theoretically and empirically equivalent. Decisions regarding which method to employ would then be wholly a matter of convenience in relation to the particular kinds of tests and subjects to be employed.[8]

Summary and conclusions. (1) In view of the absence of experimental evidence regarding the equivalence of the time-limit and work-limit methods of test administration, it seemed desirable to experimentally study the question.

(2) The Chapman-Cook Speed of Reading Tests were administered to 1090 college students so as to yield intercorrelations between the two methods of testing as well as the reliability coefficients for each method.

(3) The work-limit method was found to agree with the time-limit method as closely as each method agrees with itself. When the 'true' correlation between the two methods was computed, by use of the attenuation correction formula, r was found to be +1.00.

(4) Within the limits of the conditions of this experiment, the time-limit method and the work-limit method are equivalent.

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[8]This conclusion does not solve the perplexing problem of whether time per unit of work or work per unit of time should be employed in experiments concerning the effect of practice on individual differences. It is obvious that both units of measurement may be derived from either the time-limit or the work-limit method of test administration. Cf. J. Peterson, Thurstone's measures of variability in learning, *Psychol. Bull.*, 15, 1918, 452-455.

APPARATUS

An Improved Automatograph

By W. N. Kellogg, Indiana University

Considerable difficulty is often experienced in obtaining good results with the automatograph of the type described by Titchener[1] and Myers[2] because of the fact that (a) if the screws against which the stylus is supposed to slide are adjusted too tightly, free vertical movement of the stylus is hampered. On the

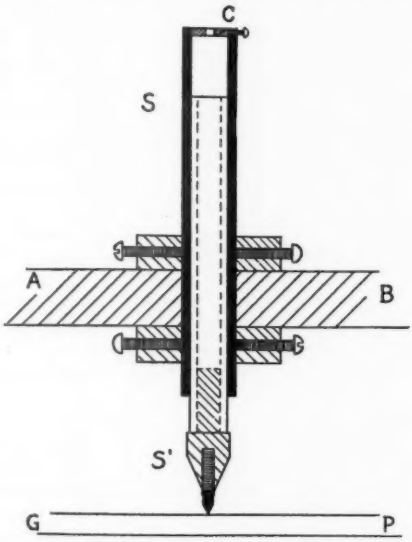

Fig. 1. An Improved Automatograph

other hand, if these screws are too loose, the horizontal play of the stylus makes clicks, when the direction of movement is changed, which are magnified by the resonance of the automatograph board so that *S* becomes aware of the slightest tremor. (b) The stylus frequently fails to run smoothly upon the recording paper, again forcibly apprising *S* of movements which would otherwise be unperceived. In the attempt to obviate one or the other of these drawbacks we have tried a glass stylus with ink writing point, a special stylus constructed of

[1]E. B. Titchener, *Experimental Psychology, Student's Manual, Qualitative*, 1901, 95 f.
[2]C. S. Myers *A Textbook of Experimental Psychology*, Pt. ii, 1911, 101 f.

polished wood which would not resound, various kinds of recording pens and inks, including the fountain pen furnished by Stoelting, and tracing on smoked papers with various forms of recorders. Success was only partial, however, until an entirely different type of stylus and writing principle were adopted. These are illustrated in the accompanying diagram (Fig. 1).

A-B represents the automatograph board with the usual wooden blocks above and below it through which run the 8 screws (4 of which are shown) that center the stylus. The stylus hole is drilled to fit a piece of heavy brass tubing (18 mm. in outside diameter and 14 cm. long) which is rigidly secured in the hole. A small disk with air hole is fastened in the top of this tubing at C. A similar piece of heavy tubing, S-S′ (2.5 mm. in thickness), which serves as the sliding element of the stylus, is machined to move easily within the outer tube *but without horizontal play*. The writing end at S′ is cut from a piece of hard wood; a machine screw is then inserted in the point of the cone and the end of the screw is turned down on a lathe to produce a fine but rounded point (black in the diagram). The wooden end is thereupon plugged into the lower opening of the smaller brass tube to complete the stylus.

Recording is accomplished by causing the stylus to write directly upon a heavy piece of plate glass (GP) which has previously been coated with Bon Ami. The weight of the stylus is sufficient to keep it continuously upon the plate and the use of glass instead of paper as a writing surface insures perfect smoothness and the elimination of 'catches.' If desired, the records can easily be made permanent by placing a piece of sensitized paper beneath them and printing after the usual manner of making blue prints, or by putting the glass upon a black background and photographing.

A New Electrode for the Hathaway Galvanic Reflex Apparatus

By Christian A. Ruckmick, University of Iowa

While the Hathaway apparatus is still in process of improvement, in the author's judgment it has one outstanding feature in that it eliminates battery and polarization effects in the electrodes because it sends through the portion of the body to which the electrodes are attached an alternating instead of a direct current. This does away with a cumbersome and bothersome type of electrodes and permits the substitution of a simple and convenient form, since metallic connection may be safely made directly with the skin. The electrodes furnished with the apparatus, however, possess certain disadvantages because the pressure and probably the area of contact cannot readily be kept constant for any one *S*, especially not for any length of time. If between two readings on the apparatus *S* is required to perform exercises or to busy himself otherwise away from the apparatus, succeeding attachments of the electrodes cannot conceivably be made in such a way as to insure uniform pressure or equal area of contact throughout the series.

The author has consequently devised the electrodes shown in Fig. 2. To a bandage of soft leather (E) 15 cm. long by 6 cm. wide are attached two straps (C) 12 cm. long by 1.5 wide, which fit into buckles (B) attached to the opposite end of the bandage by short straps extending out about 4 cm. from that

end. The long straps (C) are sewed on the under side of the bandage (as illustrated) at a point about 3 cm. back of the free edge of the bandage at E. This allows a certain amount of overlapping of the edges of the bandage when accommodated to small hands. The bandage is cut out at D to fit around the thumb.

G is a flap sewed on the bandage at one end with a free edge at the other end. Between it and the bandage a piece of sponge rubber, about 1.5 cm. thick in the middle and tapering towards both edges in the lengthwise direction, is inserted. The metallic electrodes consist of very thin flexible copper 32 mm. square (A,A′) with rounded corners, mounted about 3 mm. apart and rather nearer the upper edge of the flap to bring them into a central position on the palm when the bandage is turned over face down on the skin. They are attached by

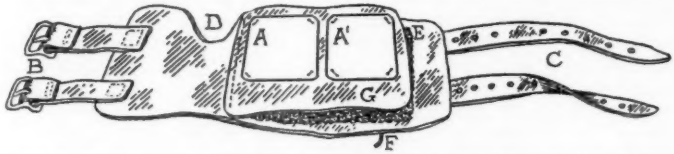

FIG. 2. NEW ELECTRODE FOR THE HATHAWAY GALVANIC REFLEX APPARATUS

means of waxed linen thread at the corners. F shows one of the connecting leads of fine woven copper wire, filled with solder at the end to prevent fraying. These emerge through slits in the bandage and serve also to keep the rubber sponge in place. The area of the electrodes equals that of the electrodes furnished with the apparatus.

When the bandage is strapped to the hand with electrodes resting on the palm it is comfortably worn even when the straps are fastened tightly enough to insure a full contact. The rubber sponge then acts as a cushion. The usual process of wiping both electrodes and the palm of the hand with ethyl alcohol is followed. After a few minutes a saturated condition of humidity, due to natural perspiration from the palm, is reached and maintained.

The wires to the apparatus terminate in clips which are fastened to the leads from the electrodes. This allows *S* to move about between successive readings without disturbing the original area, place, and pressure of contact. The bandage has been tried out with success and is adjustable to the smallest as well as the largest hands found among students in the laboratory.

A TRIDIMENSIONAL MAZE

By D. P. BODER, Lewis Institute, Chicago

The tridimensional maze described here was devised for the study of motor learning. It is made of 3/8-in. brass pipes—ordinary gas fixtures—which are screwed and soldered together in the form shown in the accompanying diagram (Fig. 3). The pipes are carefully fitted so that the joints are smooth on the inside. A 3/16-in. steel ball-bearing may therefore be rolled around freely within the system without catching or becoming lodged. Openings are left, through which the ball may fall, at A, B, C, D, E, and F.

S drops the ball at A and tries by tilting and tipping the maze in this and that direction to 'run' the ball through the pipes and to bring it out at some one of the holes previously designated as the goal. Problems may be set of varying degrees of difficulty. *S* may be instructed to roll the ball out at B—a simple task requiring only a few movements; or he may be instructed to roll the ball out at C, D, E, or F—tasks of increasing difficulty, requiring more and more carefully executed movements and delicate adjustments. *S* must know in the more complex problems where the ball is within the maze, and also how to coördinate his movements so as to avoid the culs-de-sac, *i.e.* the wrong tubes and holes.

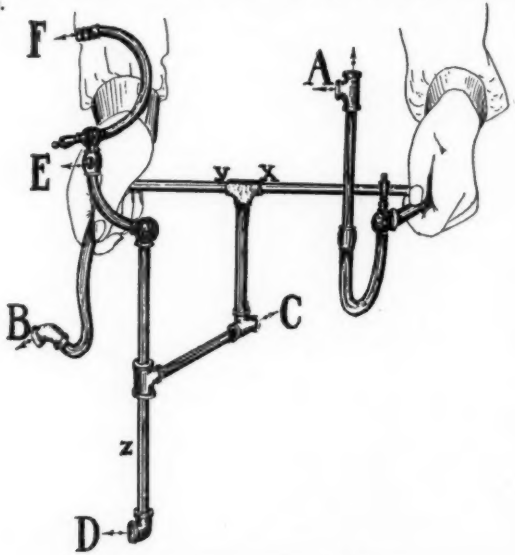

FIG. 3. TRIDIMENSIONAL MAZE

Passage of the ball into the wrong tube may be corrected, but out of one of the wrong holes means starting again at A. Performance may be measured (1) by the number of attempts made to attain success, (2) by the time required for the completion of the problem, or (3) by a combination of these two methods.

The maze is so constructed that it may easily be modified. *S* may be required to alter the position of his hands in holding the apparatus, or sections of pipe may be added, or subtracted, or interchanged. Pipe z, for example, may be removed and placed at F, or pipes x and y may be interchanged. Any change in the apparatus, either in the position of *S*'s hands or in the form of the maze, causes a shift in the center of gravity and necessitates relearning. The interchange of pipes x and y, since the ball is to be placed in the maze at A, changes also the direction of the primary 'run' and thus further breaks up the kinaesthetic pattern.

Demonstrational and Experimental Devices

By Albert Walton, University of Washington

I. A Vertical Maze for Lecture Room Demonstration

The need for ocular group-demonstration of an animal in the various stages of learning has been met by the construction of the *vertical maze*, the essential items of which are shown in Fig. 4. It is 44 in. high and 42 in. long and is built of ½-in. pine. Since the pathway of the maze is only 4 in. deep and enclosed in screening with a ½-in. mesh, the progress of the animal is visible from start to finish and from all parts of a large hall. We have used it on an elevated stage before classes of five hundred students. The revolving cage is weighted so that an opening to the wheel is directly before the animal. The exit from the wheel is at the top of the other side. The rat turns the wheel through a half-revolution and leaves as soon as the exit has come down to floor level, when the weight returns the cage to its former position ready to admit another rat.

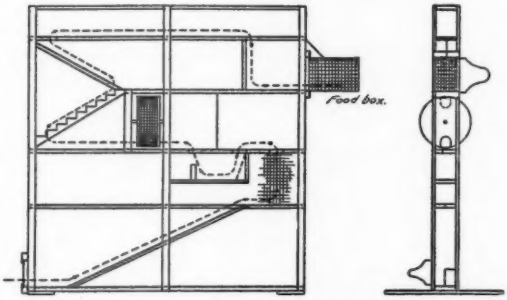

Fig. 4. Framework of Vertical Maze
Covered with a 1/2 in. hardware cloth. Dotted line shows true path to food box.

The food box is a wire cage hung on a maze by a single hook and is so long that when the rat goes to the outer end for the bait a door can be closed behind him, the cage lifted off, and the rat returned to his nest box. A trained rat will make the run in about 10 sec.

After two or three trained rats have run the maze, two or more naïve rats are introduced to find their way as they may. They seldom reach the goal during the lecture period, illustrating in their behavior all of the factors which have been treated abstractly in text and lecture.

The training of the rats for demonstration purposes involves little expenditure of time or energy. The maze is taken to the animal laboratory a week before the demonstration and is placed so that the rats can enter it directly from their nest box. By placing all food which they receive during the week in the food box of the maze while keeping the drinking water at the nest box their frequent passing from end to end of the maze is insured and they quickly acquire a mastery of the true path.

For cleaning purposes and to obviate the necessity of waiting for the naïve rats to solve the problem, one side of the maze is made removable, the screen being tacked to a frame instead of to the maze itself. The whole maze is light enough to be easily carried by one man and as it is a self-contained unit it requires no assembling but is always ready for use.

II. Conditioning Illustrated by an Automatic Mechanical Device

For purposes of demonstration in introductory psychology we have devised and constructed a self-contained, self-acting mechanical toy which completely illustrates the setting up of a conditioned response to a substitute stimulus.

The operation involves merely the bringing of a toy rabbit (substitute stimulus) into the 'field of vision' of a small doll which thereupon raises both

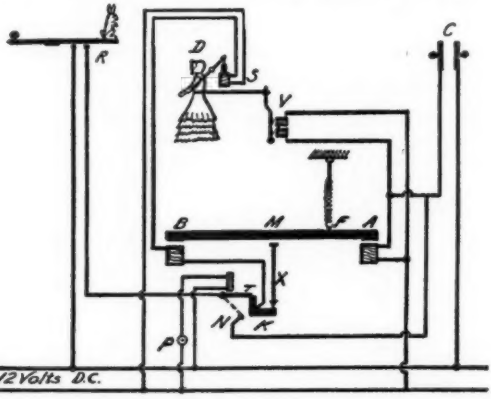

Fig. 5. Wiring Diagram for Conditioned Reflex Machine

arms toward the rabbit as an 'approach reaction.' If, on the other hand, a loud noise (original stimulus) be caused by banging together two blocks of wood the doll is caused to tremble violently. The doll follows the Pavlovian dictum that no conditioning takes place when the substitute stimulus is presented after the original or adequate stimulus. If the loud sound be made first and either discontinued or allowed to persist and then the rabbit is presented, no conditioning takes place. Even if the doll is still trembling when the rabbit approaches, the trembling is somewhat reduced (probably by reason of the 'distraction' involved), but there has been no conditioning. If the rabbit be again presented alone the old approach, reaction is made just as before. If, however, while the approach response is being called out, the loud sound be made, the arms fall to the side and trembling ensues. The rabbit being withdrawn and presented again now elicits no 'approach response,' but does bring on the fear reaction of trembling. Fortunately, unconditioning can be effected simply by pressing a push-button.

Only the rabbit and the doll appear in the performance, the synapses and ganglia involved being behind a partition. The effect on the class before which the demonstration is made is one of pleased surprise which aids in fixing the principles involved in their own response mechanisms by virtue of the very method which the device illustrates.

The mechanism involved is shown with all wiring connections in Fig. 5. The central device is the horizontal arm *BMFA* which is balanced on the floating, spring-suspended fulcrum *F*. When the loud sound is made by the two blocks at *C*, the circuit is closed through the vibrator *V* and the magnet coil under *A*. The doll trembles and arm *BMFA* is tilted by being drawn down at end *A*. End *B*, due to the unequal length of the two arms BF and FA, is raised out of the field of the magnet under *B* so that if the rabbit *R* be moved, in such a way as to close the contact under it, magnet *B* has no effect.

If, now, instead of presenting the stimuli in this order, the rabbit be moved toward the doll first, the contactor on the under side of the slide on which the rabbit is mounted closes the gap at *R* and current is sent through the trip *T* and the bell-crank *K* to the magnet under *B* and the solenoid *S* in series with it. End *B* is drawn down and the doll's arms are raised toward the rabbit.

Due, however, to the shortness of lever arm *FA*, the end *A* is not raised out of the field of its magnet, so that, if the blocks be now banged together at *C*, magnet *A* will draw its end down as before. With either end of the bar up and the other down its center remains stationary or may be slightly raised. In any event it does not touch the tripping bar *X*. If both ends are drawn down at once, as is done when *B* is depressed first, the mid-point *M* is drawn down on *X*, which is depressed enough to move the bell-crank *K*, allowing the trip *T* to drop off the bell-crank down onto a new contact *N*. Current from *R* is now diverted from *B* and *S* to *A* and *V*; the doll drops its arms and begins to tremble. From this event on, either clapping the blocks at *C* together or closing the rabbit contact *R* will actuate *V*, solenoid *S* which formerly raised the doll's arms being cut out of circuit by the breaking of the contact between the trip *T* and the bell-crank *K*.

Pressing push-button *P* resets the trip T and the apparatus is ready to repeat. In the apparatus as built, the bar *X* is dispensed with, the midpoint of bar *BMFA* striking the bell-crank *K* directly, an arrangement not easily shown in diagram since it is mounted at right angles to the plane of the sketch. The tension in spring *F* is regulated by thumb screw adjustment as is the movement of the end *A*.

III. Making the Ergograph Self-Recording and Integrating

The use of the ergograph is less common than it was in the early days of its development, not so much because it has yielded all the information which can be secured by its employment as because, in order to obtain further information, a cumbersome set-up and a wearisome process of observation and counting during the operation are required. To obtain a record of fatigue of the finger with a kymograph necessitates the lifting of excessive weights so that the fatigue may occur rapidly enough to be within the limits of the short kymographic record. The kymograph is separate from the ergograph so that each set-up is temporary and not easily portable; alignment is easily destroyed. There is also the necessity for counting both the number of strokes

shown by the curve as drawn and the number of times the meter tape has turned its length during the process. With light weights or strong fingers these two items mount to values that may easily become burdensome.

To obviate all of these difficulties we have applied the attachments shown in Figs. 6 and 7, using as a base the standard ergograph. The object was to make a self-contained portable unit which would record the strokes both as to total number and as to individual length and variation, and which would integrate the distance through which the weight has been lifted, without imposing any appreciable additional work upon the subject.

The devices used for accomplishing these factors involve three separate mechanisms; the stroke-counter, the graphic stroke-recorder, and the distance-integrator. The ergograph with these attachments is mounted on a board 4 ft. long and 11.5 in. wide, which also carries the usual arm-rest. Excessive swinging of the pendent weight when the periodicity of the strokes

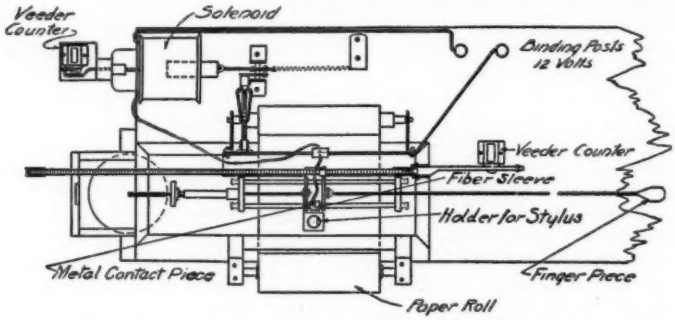

FIG. 6. SELF-RECORDING AND INTEGRATING ATTACHMENTS APPLIED TO THE ERGOGRAPH
(Plan View)

happens to synchronize with the normal pendulum cycle of the suspended weight is prevented by a cage or sort of 'elevator shaft' built at the end of the board as shown in the sketches. Affixed to the carriage of the ergograph is a holder for a stylus fountain pen. The paper on which it writes is 6 in. wide and rests smoothly on the iron surface of the instrument as it passes from the supply roll on one side of the machine to the take-up roll on the other. At each stroke of the carriage and pen the paper advances about 1/10 in. so that a continuous ink-drawn record is made showing the length of each pull and the variations in the series.

The take-up which advances the paper is a ratchet wheel 3 in. in diam. with 100 teeth, which is operated through a linked pawl by the action of a solenoid with a movable core. This coil is of about 300 turns of No. 18 copper wire and operates satisfactorily on 8–12 v. Current is supplied to the solenoid each time the carriage is pulled forward and only then. The movement of the core is transmitted through a bell crank to the pawl on the ratchet, a check engaging the teeth to prevent backward motion. At the rear of the solenoid is a spring

bumper to take up the jar. The motion imparted to this bumper is utilized to actuate a Veeder counter for recording the number of strokes made by the subject.

To provide one and only one impulse to the solenoid for each forward motion of the carriage a fiber sleeve is mounted on the round slide-bars which carry the carriage. Being inserted between the jaws of the carriage this sleeve must move forward and back with it but being light and loose and well lubri-

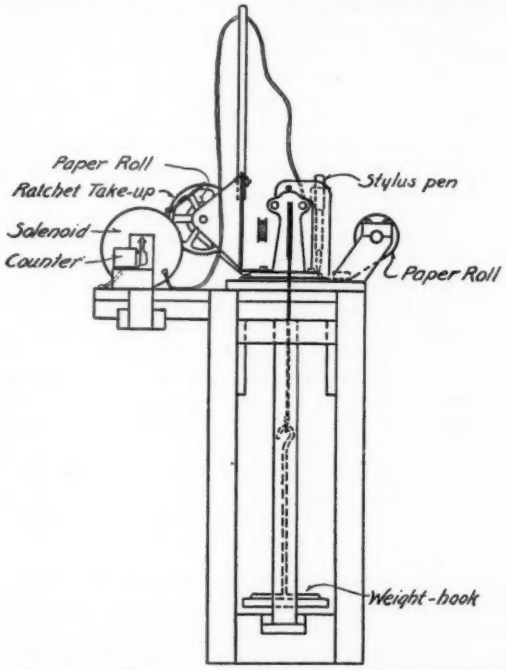

FIG. 7. STANDARD ERGOGRAPH WITH SELF-RECORDING ATTACHMENTS (End View)

cated it does not add appreciably to the work involved in operating the machine. There is play, or lost motion, of about 1/16 in. between the sleeve and the carriage. At one end of the sleeve is a metallic contact to which is soldered a flexible wire which runs to the solenoid. The other pole of the solenoid is wired to one of the two binding posts. From the other post a wire runs to the frame of the machine. Since the fiber is an insulator no current flows until the carriage is moved forward a sixteenth-inch, when contact is made between the carriage itself and the metal projection mounted on the sleeve. When this occurs a circuit is completed through the frame of the ergograph and the sole-

noid is actuated, moving the paper forward one notch and scoring one stroke on the counter. The circuit is broken as soon as the carriage starts back in answer to the pull of the main weights. Since this is always the last stroke which the machine makes, the solenoid is automatically out of circuit when the machine is not in use. When the circuit is broken a spring attached to the core of the solenoid pulls it back out of the coil, releasing the counter and resetting the ratchet ready for the next stroke. The carriage cannot be operated as rapidly as the solenoid works. The subject may therefore move his finger as rapidly as he desires without interfering with the take-up motion or the count of the strokes; nor is it necessary, so far as the mechanism is concerned, to employ any regularly repeated interval. The take-up works once for each forward stroke regardless of its length provided it is at least 1/16 in. The work of pulling the paper from the supply roll, under the two guides and the pen, and winding it up on the other side, and of actuating the Veeder counter is not done at the expense of energy on the part of the subject but entirely by current from the battery.

To record the number of times the meter tape is moved its own length a slight bump or projection is made at the zero mark. This offers no obstruction to the pawl which moves the tape on the standard machine but it is sufficient to displace a roller which is pressed up against the tape as it passes over one of the small pulleys on which it normally runs. In the machine we are using, this projection is made by rolling up the end of the tape into a cylinder about 1/8 in. in. diam. and sewing it down firmly to the tape itself. When the tape has gone around once, this bump depresses the roller which is mounted on the short end of a rocker-arm, the other end of which is connected to another Veeder counter so as to move it forward one number each time the tape has advanced one meter. To prevent undue jar on the counter-arm the link connecting the rocker-arm to the counter is made a spiral spring of sufficient strength to operate the counter with an easy stroke, regardless of the speed of motion of the rocker arm. The work of operating this arm is, of course, not imposed upon the subject since the tape moves only on the back stroke under the impulse of the weights.

The whole device may be picked up as a unit and placed on any convenient table or desk, connected to the battery circuit and so be ready for use immediately without adjustment or alteration. In a series of experiments recently made in our laboratory this machine was used to record over 30,000 strokes without appreciable wear or depreciation.

APPARATUS NOTES

A Technique for Screened Observation

In the course of an experiment on the judgment of projected motion, we desired to screen *E* and yet permit him to observe *O*'s face and also a spot of light used as an indicator of *O*'s movements. A peep-hole was unsatisfactory due to its small field of vision.

A window 6 × 6 in. square, and covered with a perforated tin plate, however, proved very satisfactory, giving *E* an adequate visual field and yet preventing *O* from obtaining any visual clues from the movements of *E*. In fact, several *O*s stated that they were rather mystified as to how the observation had been accomplished.

The material used was a thin tin plate, perforated with holes 1 mm. in diameter and 1 mm. apart, the surface toward *O* being bright, and reflecting the light from a 100-w. bulb. Such plating is commonly used for shielding radio tubes. This perforated grid presented a bright surface toward *O* while the space behind the screen was slightly darkened by covering an adjoining window. This arrangement, however, still allowed *E* ample light. The only defect discoverable was a slight reflection if *E* wore glasses and placed his face very close to the window. This defect is easily remedied by placing the light source to one side and close to the screen.

The technique could easily be applied to windows of larger area, when a greater field of observation is required.

Ohio State University T. W. Forbes

Records of Mutilated Speech and Music

While researching upon telephonic reproduction, the Bell Telephone Laboratories have made a number of phonographic disk-records to demonstrate changes in vocal and instrumental sounds due to the elimination of one or more partial tones. Band filters are used. Vowels and notes are sounded with and without the fundamental and other low partials, and also with and without the higher overtones. Other records exhibit the damage to connected speech and music through the shortening of the acoustical spectrum above and below. One of the most instructive of the records produces in direct sequence a given sound with all components above 1000 d.v. removed and the same sound with all components below 1000 d.v. removed.

The records make useful demonstrations of the dependence of complex sounds upon the constituent parts. The Cornell Laboratory has also used them in its Undergraduate Laboratory for instruction upon composition and clang color and for memorizing without intent to learn. The latter experiment rests upon the fact that certain sentences are repeated over and over by the disks without calling attention to their verbal significance.

M. B.

A Standardized Hypnotic Technique Dictated to a Victrola Record

The apparatus consists of a double-faced (12-in.) Victrola record. The technique demonstrated is the usual 'sleeping' method employed by Bernheim and the Nancy School.

On one side of the record the operator simply talks of sleep in the usual way. He dwells upon the subject's inability to move the right arm and to open the eyes. This side of the record requires 4 1/4 min. It is given first. Just before it ends there is a pause of 5 sec., after which the record wakes the subject by counting ten.

The second side of the record begins also by talking sleep and inserts a test of inability to bend the right arm. It also transfers the rapport from the record to another operator who is present, provided the subject has given his written consent but under no other conditions. If the rapport has not been changed to the other operator, there is again a pause of five seconds and the record again wakes the subject.

The interesting features in this record are: (1) it enables hypnotic experiments to be carried through on a standardized technique; (2) the record auto-

matically wakes the subject in the event that no operator is present; (3) should this other operator be using the record for hypnotic purposes, he simply turns over the record at the pause mentioned in part one and continues with the other side, taking over the control as described for the second part and stopping the record before it wakes the subject (it might be advisable to insert a pause of two or three minutes between the two halves of the record); and (4) therapeutic suggestions are interposed with the usual suggestions of sleep in such a way that the use of the record should not lead to bad results. The entire record is simply an effort to produce a standardized laboratory technique for experimental purposes.

Colgate University G. H. Estabrooks

Lacquers and Celluloids for Colored Surfaces

Mr. Edwin Newman reports from the University of Kansas the successful use of colored lacquers upon the metallic buttons which certain perimeters employ as colored objects. "A large variety of these lacquers have appeared on the market in colors that are both bright and saturated. Sufficient material for a hundred buttons was obtained at a trifling cost and it required but an evening to apply the lacquer. They have been used with complete success. The advantages of the lacquer are at once apparent. The colors do not fade; they may be cleaned in an instant with a damp cloth; if allowed to dry and to harden, they are practically indestructible; they are cheap; and they are easily applied. They could probably be applied also to colored papers."

The Marietta Apparatus Company has prepared celluloid disks for color mixers and other apparatus by applying a color pigment to the back surface. The colors are evenly put on and they seem permanent. As the operator's hand does not touch them, they do not soil. Possibly Newman's colored lacquers would usefully replace the ordinary pigments upon the celluloid.

M. B.

An Aid for Steady Visual Fixation

As an aid to stabilizing visual fixation the Cornell Laboratory has hit upon the use of a glass mirror. The mirror is placed in the line of regard, and the pupil of the reflected image of the eye is taken as the fixation-point. Since the mirror doubles the distance of the fixated object, it should be placed at half the focal length desired; *e.g.* if the focal distance is 1 m. the mirror should be placed 50 cm. away from the eye.

In monocular fixation, small mirrors (3-5 mm. in diam.) should be used. The exact size depends upon the focal distance. The mirror should be large enough in every case to include in its reflection the iris; for the iris serves as the background of the pupil, and thus of the fixation-point. In binocular fixation the mirror may be large enough to reflect the image of both eyes, the pupil of one being then selected as the common fixation-point; or a small mirror may be used and so placed in the line of regard as to reflect the image of but one eye, the pupil of which is taken as the fixation-point of both eyes.

By using the pupil as a fixation-point, *O* is able to 'hold' his eye, and so to report with a high degree of assurance any slip in fixation. This aid to fixation is limited, however, to light adaptation, because there must be sufficient light for the reflection of the eye in the mirror.

K. M. D.

NOTES AND DISCUSSIONS

Color Problems: The Divergent Outlook of Physicist and Psychologist

Jaensch's incorporation of the after-image in his system, the *Erscheinungsweise der Farben* of Katz, and the appropriation of visual contrast by exponents of *Gestalt* have brought color problems again to the fore in our laboratories. Recourse to the retina in the interests of certain psycho-physiological enigmas, *e.g.* the neural correlates of qualitative and intensive differences (Adrian, Troland, Hecht), has motivated other studies; while innumerable problems soluble only in psychological terms have been turned up of late by the illuminating engineer. Frequent discrepancies in the results both of these and of earlier researches are, in the opinion of the writer, due not merely to divergent outlook and method on the part of the experimenter but also to idiosyncrasies in the visual systems of the various observers.[1] In the interests of laboratory technique no less than of theory there is need of a better understanding of aberrant cases.[2] If, as the work of Hess and others renders probable, numerous gradations or variants exist both between the 'normal' and the 'color-weak,' and between the latter and the partially or totally 'color-blind,'[3] psychologically valid methods or the scaling and rating of color vision in both normal and deficient cases should be formulated, to be applied to observers in any given visual problem.

Yet when a case of unusual color vision (holding, perhaps, the key to numerous deviations from the norm) falls into the hands of a psychologist unskilled in the specialized technique of visual research or unversed in the vast literature of this special field, the would-be investigator faces a dilemma. Time is precious, and while one is orienting oneself in the problem and assembling suitable apparatus, the subject is apt to slip beyond recall. In the matter of color problems, moreover, the physicist was first in the field. The psychologist, as a late entrant, not only reaps the profits of his predecessor's labors, he suffers also the handicap of traditional misconceptions and is balked by the inevitable distortions arising when a function so complex as vision is attacked from the angle of the most abstract of sciences.

I cite an instance. Late in the spring of 1928 a case of congenital R-B-G blindness, with no detectable pathological symptoms, was brought to the notice of the writer. The retention of the primary *yellow*, with the loss of *blue*, (an

[1]For evidence and for partial recognition of this factor by physicists see: E. C. Crittenden and F. K. Richtmyer, An 'average eye' for heterochromatic photometry, *Bur. Stand. Sci. Papers*, No. 299, 1917, 96; I. G. Priest, The spectral energy required to evoke the gray sensation, *ibid.*, No. 417, 1921, 237, 252 f., 256 f.; K. S. Gibson and E. P. T. Tyndall, Visibility of radiant energy, *ibid.*, No. 475, 1923, 178 f.; and M. K. Frehafer, Preliminary note on after-images from stimuli of low saturation and short duration, this Journal, 41, 1929, 282 f. In the latter variations are reported in the hue of the Purkinje image correlative with pigmental differences in the observers.

[2]The growing importance of color discrimination in connection with traffic signaling, aviation, bacteriological tests, etc., furnishes an added incentive for research of this nature.

[3]C. von Hess, Farbenlehre, *Ergeb. d. Physiol.*, 20, 1922, 12 f., 19 f.

anomaly not easily reconcilable with the tenets of a 3-, a 4- or a 7-component theory) singled the instance out for more extended study. A rapid survey of the literature was undertaken for accounts of similar cases, for light on the possible rôle of yellow pigmentation in reducing the effectiveness of short-wave stimuli (blue), and for aid in settling upon the more important measurements and suitable apparatus and technique. Complete files of ophthalmological journals were not immediately accessible, and no succinct and reliable bibliography of the limited number of studies available in psychological periodicals was at hand.[4] Recourse was therefore had to such works of reference as J. H. Parsons' *Introduction to the Study of Colour Vision* (1915 ed.); J. von Kries' article on vision in Nagel's *Handbuch der Physiologie des Menschen*, III, 1905; and H. von Helmholtz's *Physiological Optics* (Amer. ed., 1924). G. E. de Schweinitz's *Diseases of the Eye* (1924 ed.), F. W. Edridge-Green's *Physiology of Vision* (1920), and M. Collins' recent book on *Colour-Blindness* (1925) were also consulted on special topics.

Misconceptions and false leads rising out of certain of these sources obstructed plans and retarded a grasp of the essentials of the problem until too late to apply certain significant measurements (dark adaptation and the exact comparison of center and periphery). Hence the following cautionary note to others who may find themselves in similar straits. Difficulties due to differences in point of view and terminology are perhaps inevitable; but erroneous citations and reports of experimental data in standard works of reference are more misleading because long undetected. The substitution of *a priori* deduction or stock interpretations for direct observation, and the wide currency of inferences based upon scantily verified data and unsafeguarded methods likewise call for protest. A comparison and critique of the divergent outlook and methodology of physicist, physiologist, and psychologist may clarify discussion.

Description of cases. First as to terminology and theoretical bias. Search of the texts and records for a similar case of gray-yellow vision revealed a literature obsessed by obsolete distinctions,[5] derived from the physicist's 3-component color-mixing theory. Cases are listed as trichromatic, dichromatic and monochromatic, terms admittedly non-descriptive, having reference to color-mixing equations merely. It is sheer accident that the second, *dichromatic*, squares with the current view of the commoner form of color deficiency as red-green blindness or *blue-yellow* vision (in which, as Hering demonstrated, all the spectral qualities can be duplicated through the use either of a 'yellow' stimulus

[4] S. P. Hayes' résumés of the literature on color defects in the *Psychol. Bull.* have since proved invaluable (8, 1911, 86-89; 12, 1915, 108-11; 13, 1916, 131-34; 16, 1919, 138-42; 23, 1926, 547-74). M. Collins' bibliography in *Colour-Blindness*, 1925, 227-34, while fairly full on the early history, is inadequate with respect to later experimental studies.

[5] Since the publication of the Report of the Colorimetry Committee for 1920-21 (*J. Opt. Soc. Amer.*, 6, 1922, 527-96), a slight improvement in terminology is noticeable, *i.e.* there is a tendency to replace *monochromatism* with *achromatopsia*, to separate brightness or brilliance from saturation, and to distinguish both from luminosity or the physical energy of the source. Separate curves for brightness and chroma values will perhaps follow, and the joint contribution of chroma and brightness to lamprosity or visibility (*e.g.* in flicker or equality photometry) be recognized; though the old notion of brightness as the sum or quotient of chromatic values obstructs progress.

(about 589 mμ) or a 'blue' (around 480 mμ) either singly or in combination. *Monochromatic* is, however, merely a physicist's tag, signifying that in this type of deficiency any wave-length of light can be matched against any other. Actually the vision of the subject is *achromatic*, without any color quality whatsoever. No genuine *monochromasy* in the descriptive sense, is, for theoretical reasons (the compounding of white from chromas), admissible. If discovered it is quickly cast aside as 'pathological.'[6] The preliminary search for a parallel case of 'yellow' color vision therefore proved baffling; though a wider canvass of the literature (Hess, Stilling, Köllner), plus experiment on control cases, has convinced the writer that many such cases (probably congenital) do exist, however camouflaged by the antiquated nomenclature and test methods still current.

Furthermore, although the old Helmholtzian terms 'red-blind,' green-blind,' and 'violet-blind'[7] are virtually obsolete (authorities in all camps admitting that both the first two groups are *red-green* blind) these rubrics still persist among users of the Holmgren wool test and in the backwaters of college and university instruction. The correlative terms of the physiologist, moreover, (*protanopia*, lack or non-functioning of the first or R process; *deuteranopia*, of the second or G process; and *tritanopia*, of the third or V process) are equally vicious and misleading. Lesser workers in the field of physics or physiology, accepting these terms at their face value, regard the retina as a simple sum in

[6]This is true even for Edridge-Green's 6-7 component theory, which assumes, *Gestalt*- or figure-and-ground-wise, that there must be a second hue, preferably at the remote end of the spectrum, in order to differentiate out the first. F. Allen, to be sure, representing the 3-component theory in its older, cruder form, attempts to find indirect evidence (by way of the selective after-effects of fatigue on flicker in different regions of the spectrum) for all seven possible combinations and eliminations of the three components (*i.e.* not only for R, G, V and total color-blindness, but also for R-V, G-V, and R-G); succeeding to his own satisfaction in the case of all but the last-named, V vision. His conclusions are vitiated, however, by various irregularities in his procedure and the make-up of his curves.

[7]'Red,' 'green,' and 'violet' in these compounds are sometimes taken as *stimulus-terms*, implying absolute blindness to the three corresponding sets of wave-lengths, or regions of the spectrum (in contradistinction to Hering's designation 'red-green blind,' which implies a gap in color-experience merely, *i.e.* the lack of the corresponding *color-qualities*). Again, they appear to refer simply to the absence or lack of function of the 'red,' 'green,' or 'violet' nerve-fibers of Young. Undoubtedly, in common usage, they connote an uncritical confusion of stimulus with sense quality, such as apparently inspired Brewster's curious notion of the three kinds of light, each representing all degrees of refrangibility, each coextensive with the spectrum, and superposed on the other two (cf. D. Brewster, The triple spectrum, *Athenaeum*, 1855, 1156).

In the 14th edition of the Encyclopedia Britannica (1929) in the article on Vision, J. H. Parsons admits: "It was, perhaps, natural that the three groups of partially colour-blind should be denominated red-blind, green-blind, etc., by upholders of the Young-Helmholtz theory. It has, however, had the most disastrous results, and has led to endless ambiguity. For in the obvious meaning of the terms these people are not red, green, or violet blind. They simply have different responses from the normal when their eyes are stimulated with these particular regions of the spectrum. The attempt of Von Kries to eliminate ambiguities by calling the conditions protanopic, deuteranopic and tritanopic failed in its object and seems only to have infuriated opponents of the Young-Helmholtz theory."

addition, and record their cases in terms of the hypothetically remaining pair of processes—blue-violet, or red-violet, as the case may be. Extraordinary plates, purporting to represent the color worlds of the two types—protanopic and deuteranopic—have been constructed along these hypothetical lines and even found their way into print.

Where arbitrary deduction is allowed to supplant in this fashion exact observation, yellow of course drops automatically out of the records. A derivative color, according to Young-Helmholtz premises, it disappears of necessity when either of its components, red or green, is lacking. A virtual conspiracy, therefore, exists against the admission of this quality into case descriptions; although, in the face of accumulating evidence, Parsons and others now concede, parenthetically, that it is perhaps the dominating hue in the spectrum of the commoner forms of color-blindness. The origin and history of this minor skirmish between exact observation and theoretical bias, as found in some of the older texts (*e.g.* Jeffries),[8] are illuminating. Dalton, Pole and others, though pronounced now red-blind, now green-blind (*i.e.* possessed only of G-V or R-V vision) by ardent advocates of Young or Helmholtz, stood out stoutly for the dominance of yellow in the warm end of their spectra; the dispute finally terminating in decisive evidence furnished by the discovery of cases of monocular and acquired color-blindness. Helmholtz himself in the second edition of his *Optik* (1892) abandoned the earlier *Ausfall* or *reduction* theory, accepting from Fick and others the suggestion that no element is missing or out of function in partial color-blindness. Rather the stimulus curves for the R and G elements have become identical, so that the two are always correlatively stimulated, evoking the derivative quality yellow instead of red or green.[9]

The complication thus introduced, however, is tantamount to positing a true 'yellow' process (a totally new mechanism, that is, susceptible to a wider range of wave-lengths). It detracts, therefore, from the vaunted simplicity and economy of the theory, and is consistently ignored by the majority of physicists and by numerous physiologists as well. The latter, if they worry at all over the refractory facts of color-blindness, are inclined to refer them to some mythical derangement of cortical centers—forgetting, apparently, that the fovea has bilateral representation, and that any malformation or lesion, if it is to explain binocular color deficiency, must occur symmetrically in the two hemispheres. Ladd-Franklin's retinal solution of the difficulty, referring the retention (or emergence) of the fourth component (yellow) in the periphery and in color-blindness to lack of development or reversion of the color molecule, is a shade less intricate. Her reds and greens, however, are not the vanishing pairs of the periphery or of color-blindness. When shifted to represent the latter, they yield not yellow but gray on mixture. Nor does she attempt to grapple, except pictorially and verbally, with the shortened spectrum and shift in luminosity of protanopic or 'protoxanthic' vision.

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[8]B. J. Jeffries, *Colour Blindness: Its Dangers and Its Detection*, 1879. See also M. Collins, *Colour-Blindness*, 1925, 3 f., for a brief historical survey of this dispute.

[9]See J. von Kries in Nagel's *Handbuch*, III, 1905, 203-4. Von Kries himself, however, refuses to subscribe to the change, aware that it entails more serious complications than those that it is designed to obviate.

A third stumbling-block in the way of the psychological reader, obscuring evidence and precluding comparison of data, is the physicist's sheer inability, on theoretical or other grounds, to distinguish saturation from brightness differences.[10] This not only renders dubious results obtained by the *Eichung* of the spectrum (the matching of any portion by mixture of wave-lengths from two others), or by the Rayleigh equation (the mixing of 'red' and 'green' to match yellow), but it also falsifies the stock description of 'protanopic' and 'deuteranopic' vision. Accounts, which are apparently authentic, are on record of cases of red-green blindness in which the long-wave end of the spectrum is not merely colorless but darkened, *i.e.* the range of effective light stimuli curtailed. Unable to distinguish between *dulled* and *darkened*, the formula-ridden physicist (and others) is moved to set these cases over against the commoner ones, construing deuteranopia in parallel with protanopia, and arguing that, since red is darkened for the latter, green must be darkened for the former. The distinguishing mark of 'deuteranopia' therefore becomes the matching of greens with darker tones than those demanded by the normal eye.[11]

A milder but equally misleading form of this delusion is widely current, the belief that the partially color-blind distinguish reds and greens primarily by brightness. Only a wide acquaintance with the literature and a careful study of cases will free the investigator from this obsession, which tends continually to snarl up procedure and confuse interpretation. Actually the common color-blind *O* appears to distinguish red from green primarily by saturation. 'Reds' are relatively highly saturated yellows (somewhat darkish and approaching golden-brown); 'greens' are low saturations (tannish). A shift in saturation without change in brightness suffices to confuse the *O*, and lead him to alter his judgment from 'red' to 'green,' or *vice versa*; whereas a considerable range of brightness would not shake his confidence.

Pigmentation and types of color deficiency. Search for the facts relative to macular pigmentation and its possible bearing on unusual types of color vision proved equally baffling. The passages referring to visual purple and macular yellow in Parsons' widely quoted *Introduction to the Study of Colour Vision*

[10] J. H. Parsons (*Introduction to the Study of Colour Vision*, 1924 ed., 31 f.) is plainly puzzled by the use of the term saturation (or *Sättigung*) in the literature. His only definition is in terms of purity or *degree of mixture with white light*. H. Köllner, usually fairly alert to psychological distinctions, in *Die Störungen des Farbensinnes*, 1912, 2, also defines saturation by reference to transitions of color tones toward white; brightness (*Helligkeit*) by reference to transitions toward black!

[11] *E.g.* De Schweinitz, *op. cit.*, 513, states (on the authority of W. Thomson) that the red-blind confuse light red with dark green and black; the green-blind, dark red with light green (a stereotyped opposition fundamental in Holmgren wool-test procedure). In Monroe's *Cyclopedia of Education*, 1911, R. P. Angier, who worked with Nagel, makes a similarly misleading statement to the effect that "all reddish colors to the red type look darker than greenish colors, whereas for the green type the greenish colors are darker and the reddish lighter," leaving the reader doubtful whether these brightness alterations represent departures from normal vision or from the contrasted type. One suspects, however, that the opposition is largely inferential, the cropping out of the old logical fallacy of contradictory opposites, with little factual basis. Color-mixing equations afford little clue as to which of the two components is *brightened* or *darkened*.

(1915 ed.) were especially perplexing. Not until a year later when Hering's original articles in *Lotos* for 1884 and 1885, Von Kries' Über Farbensysteme in the *Zeitschrift* for 1897, and Trendelenberg's researches on visual purple were available, was the unreliability of Parsons' account thoroughly apparent.[12] For example, the curves for scotopic luminosity and for the bleaching value of different wave-lengths for visual purple are interchanged, and Trendelenberg's results for the rabbit wrongly referred to the frog (p. 14)—errors corrected in part only in the 1924 edition. As for Hering's experimental study *Über individuelle Verschiedenheiten des Farbensinnes*,[13] ascribing the limiting types of R-G blindness (*i.e.* the 'red-blind' and the 'green-blind' of the older theories) to differences in macular or lenticular pigmentation, this is twice wrongly transcribed by Parsons (p. 259 f.). For example, Parsons' statement, "Herr B, regarded as yellow-sighted, with little pigmentation, Herr S, relatively blue-sighted, with greater," exactly reverses the relationship posited by Hering—to the confusion of the reader, already puzzled by the paradox that the wave-lengths absorbed by 'visual purple' are enhanced in effectiveness, those absorbed by 'yellow pigment' rendered less effective as visual stimuli. Hering is further erroneously quoted by Parsons as declaring that "his own extra-foveal matches stood to foveal as deuteranopic to protanopic"—an exact reversal of the facts.

Parsons also states that Hering found that his 'anomalous trichromates' (examined in parallel with an array of divergent dichromates) fell into two groups, one making the luminosity match of spectral red as 1.15:1, the other 7:1 For the correction of this peculiar twist to Hering's data, the reader is referred to the original (p. 162 f.). Hering found his small group of 5 apparently continuous; he was, moreover, concerned with the yellow valence, not the luminosity of red (two things not even approximately identifiable, except for a physicist convinced that luminosity is merely the summation of color valences, and unable to distinguish chroma from brightness). Finally, he was not matching but *mixing* red with blue to get a pure (*i.e.* non-yellowish) primary. The figures represent the necessary proportions of red to blue for the two limiting members of his group, Herr B (yellow-sighted) and Herr S (blue-sighted).

Additional misstatements by Parsons refer to Hering's choice of primaries (declared by Parsons to be 'bluish red' and 'bluish green'); to the wave-lengths of the corresponding stimuli, and their varying position for protanopes and deuteranopes—all in total disregard of Hering's choice of 'pure' (*rein*) red and green (crimson and verdigris—neither bluish or yellowish), and his refusal to fix stimulus relationships exactly, owing to the difficulty of ruling out adaptation and individual variation in pigmentation.

Though some of these errors are doubtless undetected slips of the pen, it is difficult to avoid the conclusion that Parsons either never consulted Hering's original articles, trusting to inferior transcripts, or that he was too indifferent to the 'opponents' theory of color vision to clarify his notions of Hering's postulates and procedure *versus* the deuter-protanopia distinction. The effect of these errors and inaccuracies is magnified by their uncritical acceptance and widespread citation.[14]

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[12]Some of these errors are perhaps derived from similar errors in Von Kries' article on vision in Nagel's *Handbuch*.

[13]*Lotos*, n. F. 6, 1884-5, 142-98.

[14]Cf. *e.g.* Collins, *op. cit.*, 44-45.

Further highhandedness and bias in dismissing Hering's use of variable pigmentation (or blue- and yellow-sightedness) as an explanatory principle in partial color-blindness appears in a later passage,[15] where data from Von Kries' *Über Farbensysteme*[16] are quoted. In the article cited, Von Kries reports a long and varied series of observations with spectral mixtures, attacking Hering's contention that 'protanopic and deuteranopic dichromates' represent (in analogy with 'protanomalous and deuteranomalous trichromates'), not two disparate reduction forms of normal vision, but a single group, characterized in common by a lack or weakness of the R-G mechanism, and distinguishable (if at all) by anomalies of pigmentation which may or may not occur in continuous series. The *'cold curves,'* however, reproduced by Parsons as rebuttal evidence, and apparently demonstrating a greater divergence in sensitivity to short wave-length stimuli (*i.e.* those affected by yellow pigmentation) between two protanopes than between protanope and deuteranope, are those derived from Von Kries' preliminary observations, the erratic and unreliable character of which is obvious upon mere inspection.[17] Later and more exactly standardized determinations showed, in fact, far less significant deviations. Furthermore, extraordinary irregularities of procedure render the *'warm curves'* reproduced for the two sets of *O*s non-comparable (except for confirmed Helmholtzians of the old order, who identify all the long-wave third of the spectrum with the 'red' process). For example, the data for these 'warm' curves, designed to show the shift of the peaks of stimulus sensitivity in deuteranopia toward the long-wave end of the spectrum, in protanopia toward the short-wave end (also the higher maximum of the latter),[18] were secured by using as a standard for comparison a stimulus wave-length of 651 mμ (orange-red) for the first group, of 589.1 mμ (yellow) for the latter!.

Other observations cited by Parsons are designed to demonstrate the wide separation of 'protanopes' from 'deuteranopes'; the former constituting a homogeneous group, characterized by relative insensitivity to 'red' stimuli, the wave-lengths for which, as Von Kries points out, are precisely those unaffected by the variable yellow pigmentation used by Hering to explain deviations within his unified 'red-green-blind' group. If, however, we supplement the variations in 'visual yellow' posited by Hering with variations in 'visual purple,' this objection vanishes. It is not improbable, *i.e.*, that 'protanopic' (or, better, *scoterythrous*) vision, either in partial color-blindness or in color-weakness, is characterized not merely by diminished yellow pigmentation, but also by an *increase in the amount or effectiveness of 'visual purple.'* This pigment (really *rose*) with a maximal absorption somewhere between 511 and 554 mμ, by its selective reflection of long wave-lengths (along with a few of the shortest) may

[15] *Op. cit.*, 168 f. in the 1915 edition; 176-81 in the 1924. For an even more offhand and unfair statement of Hering's position, see the *Ency. Brit.*, 14th ed., article on Vision, p. 210.

[16] J. von Kries, *op. cit.*, *Zsch. f. Psychol.*, 13, 1897, 241-324.

[17] A source admittedly weak in high frequency radiations was used. Moreover, each of these widely circulated curves is based apparently upon a set of single unchecked observations for one *O* only; in all, data for two protanopes and two deuteranopes are used! (*Op. cit.*, 252, 273.)

[18] Correlative, according to the Helmholtzian, with the dropping out of the green- and red-sensitive elements respectively.

account for the relative ineffectiveness of the lower frequencies, either as brightness or color stimuli. Hence the loss of the brightness and yellow valences of the long-wave end of the spectrum, superadded to loss of the red primary in the case of R-G blindness. Strangely enough, this explanation, suggested by the marked resemblance between the luminosity curves for the 'protanope' and for normal twilight vision (along with the loss of 'red' sensitivity in the latter), appears not to have occurred to Von Kries, whose name is associated especially with rod and visual-purple theories. Von Kries, however, has assigned to visual purple primarily a peripheral function, whereas the current drift of scientific opinion is against the rigid exclusion of rod and purple-pigment phenomena from the fovea; a shift in harmony with the above conjecture.[19]

Not only, therefore, does the physicist's rule of laboratory economy, the 3-component mixing-law, when metamorphosed into a physiological theory and grafted on the retina, work havoc with exact observation and description, lending itself too readily to *a priori* inference. It also appears linked with innumerable irregularities of method, which vitiate not merely the conclusions but also the raw data of its adherents. Naturally apparatus-minded, the physicist (and in his wake the physiologist), having carefully calibrated his sources, is disposed to regard the observing *O* as a cipher, the retina as little more than a cover-glass interposed in the path of a light-ray. Not only, therefore, does he feel free to use *widely different wave-lengths for normal and comparison curves*, substituting at will an orange stimulus for a red or yellow, an olive for a green (Allen, Von Kries, Laurens and Hamilton),[20] but he is also *negligent in recording the exact retinal area stimulated* (foveal or parafoveal), thereby making it difficult to allow for the elimination of short-wave stimuli by macular stimulation, or of

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[19]Cf. F. W. Edridge-Green's *Physiology of Vision*, 1920, 43 f. Differences in pigmentation may of course be of pathological or accidental origin; but the suggestion that the tendency to excess in either pigment (yellow or purple) may represent a unit trait, separably heritable, with a survival or protective value relative to certain primitive environments (southern desert or foggy northland), yellow pigment absorbing excess ultra-violet radiations, visual purple sensitizing the eye to dim reflections, is not without factual support. The occurrence of yellow oil-drops in the retinae of hawks and falcons is suggestive. Also the hereditary transmission of non-pathological hemeralopia through several generations in families in southern France (Nettleship). Exact experimental demonstration is, however, as yet wanting.

[20]See H. Laurens and W. F. Hamilton, Sensibility of the fatigued eye to differences in wave-length in relation to color-blindness, *Amer. J. Physiol.*, 65, 1923, 569-584. In these and other recent studies, the derivative or secondary character of yellow is settled to the satisfaction of the experimenter by using long wave-length 'greens' and short wave-length 'reds'—both stimuli of a relatively high yellow valence (Allen, Laurens). Frequently a stimulus actually containing many 'yellow' rays is used (S. Hecht, On the binocular fusion of colors and its relation to theories of color vision, *Proc. Nat. Acad. Sci.*, 14, 1928, 237). The statistics collected, designed to deal a knock-out blow to the pretensions of yellow as an *Urfarbe* (the psychological brief for which is erroneously supposed to rest on its phenomenologically unitary character) frequently furnish data which with greater cogency and logic might be applied to the support of a 4-component theory.

long-waves ones by visual purple.[21] Thirdly, the precise conditions of *adaptation* or of *general illumination* during experimentation are often uncontrolled or unrecorded, also the exact duration of stimulation and of the interval between observations, or their order (from left to right of the spectrum). In the case of dark adaptation, there is frequent failure to take account of the selective absorption of visual purple. Slips of this sort vitiate many of the conclusions of recent work on color fatigue (Allen, Laurens and Hamilton). The factor of *simultaneous brightness induction* from the surrounding field, *e.g.* from the darkened walls of a tube or from an artificial pupil, although it has a differential effect on different hues, is also frequently overlooked. Finally, sweeping generalizations are based upon a *single unchecked or unrepeated set of observations* by a single *O* (this holds for much of the work of Von Kries and also of Allen).[22]

Experimental methods. Lastly, as to the search for specific methods of charting the capacities or limitations of the partially color-blind or color-weak eye.[23] The value of the much-advocated Rayleigh equation, *i.e.* the mixture of homogeneous red and green stimuli to match a homogeneous yellow (preferably with the aid of the Nagel anomaloscope), is virtually nil, except to adherents of a rigid 3-component theory. Actually, instead of working with two invariables, as the latter assumes, it deals with three unknowns. An overbalance of green in a given *O's* equation may mean not diminished sensitivity to that stimulus, but heightened sensitivity to red;[24] or merely a shift due to altered absorption in accessory pigments. Furthermore, the customary procedure of increasing intensity by widening either collimator slit (and thereby decreasing the homogeneity of the mixture or of the standard stimulus) introduces erratic changes

[21]Many irregularities in recent data obtained from 'normal' *Os* might be explained if the effectiveness of visual purple within the macula, especially under the 'mesopic' conditions usually prevailing in visual experimentation (cf. W. T. M. Forbes, An interference theory of color vision, this JOURNAL, 40, 1928, 17, footnote 31) were recognized, and its variability in different *Os* determined. *E.g.* the shift toward blue or violet with dimming of the stimulus area may possibly be a visual purple phenomenon (T. Karwoski, Variations toward purple in the after-image, this JOURNAL, 41, 1929, 625-36).

[22]Even the psychologist is not exempt from this shortcoming. The value of many of the determinations published by G. Rand and C. E. Ferree (*e.g.* Chromatic thresholds of sensation from center to periphery of the retina, *Psychol. Rev.*, 26, 1919, 16-41, 150 ff.) is lessened by the fact that they are reported for a single *O* only. This holds also for the results of Karwoski mentioned in footnote 21.

[23]The standard commercial tests (Holmgren, Nagel, Jennings, etc.) are all quite definitely constructed upon 3-component premises; the Edridge-Green lantern on a 6-7 component basis. None are designed or fitted for exact diagnosis, utilizing as they do too limited a range of hues and chromas. Their specific aim, indifferently performed at best, is the selection of the 'dangerous' color-blind. The Stilling confusion figures (based on Hering premises) provides, it is true, a fairly valuable preliminary. Unfortunately the usefulness of one of the best of recent modifications has just been jeopardized by the thoughtless publication by a psychologist (Miles) of a complete and easily memorized key.

[24]Hess, *op. cit.*, 16 f. For the recent use of this equation to indicate the range and distribution of color anomalies in 'normal' vision, see H. Köllner, *Die Störungen des Farbensinnes*, 1912, 52 ff.; M. Winfield and C. Strong, The Hering color-blind apparatus and the normal Rayleigh equation, this JOURNAL, 32, 1921, 425-8 (100 *Os*); and M. Collins, The Rayleigh color equation with rotating disks, *Brit. J. Psychol.*, 19, 1929, 387-93 (200 *Os*).

in chroma and procedure. Quite different chromas and brightnesses of the standard yellow may appear in the final matches; and, as a matter of fact, widely different equations are acceptable to the same *O*.[25] The equations, moreover, probably hinge not upon the production of a new quality, yellow (Helmholtz), but upon the summation of the yellow valences of the two stimuli (Hering); the yellow, masked by either saturated complementary (in normal vision), emerging into prominence with the balancing out of the latter through mixture.

Similar criticisms hold, *mutatis mutandis*, for matching by mixture over the spectrum as a whole. Differential sensitivity to wave-length also, as ordinarily determined,[26] with no attempt to distinguish hue, chroma, and brightness differences, has no significance to the psychologist. Two other measures commonly recommended, the determination of the shortening of the spectrum and of central scotoma, are discovered to require a highly specialized technique and to be of dubious import.

On the whole, therefore, the psychological experimenter, while utilizing the recent work of the physicist in calibrating sources,[27] will find the methods of an investigator such as Hess far more suggestive and far less misleading. Hess, for instance, will never be found guilty of using as a control data secured on a different day, with unequated stimulus intensities, and wave-lengths selected from other (if nearby) regions of the spectrum (Allen). Rather he always pits a normal *O* against the *O* to be examined, testing the two simultaneously, or under identical conditions—a refinement in method inherited from Hering. A few careful quantitative studies carried out along the lines sketched in the *Farbenlehre* should go far toward clearing up the many obscurities of the subject. The limens for the four primaries,[28] the limits of the peripheral fields for brightness and color, and the brightness values of the different hues under dark and light adaptation need to be accurately determined, under carefully standardized conditions. Artificial daylight, a neutrally tuned eye, calibrated stimuli of given areas, at set distances, on a predetermined background, and with controlled exposure times, should be the invariable rule.

The experimenter in psychological territory, moreover, should use to the full his knowledge of the complexities of the psychological organism; setting precedents in methodology by his control of expectation, his repetition and reversal of series, his allowance for tuning and after-images. The shift of hues with alteration of objective intensities, with simultaneous induction, or mal-accommodation should always enter into his calculations. Certainly he will never commit the mistake of the mathematically minded physicist, anticipating a fixed relation between the wave-lengths of complementaries (Cook, Karwoski),

[25]See, *e.g.*, Collins, *op. cit.*, 110 f. The misleading statement recurrent in Parsons (in support of the 'reduction' hypothesis of color-blindness) to the effect that both normal and color-blind accept the same equations apparently refers merely to the *identity* of the components, and not at all to their arithmetical proportions in the equation as such.

[26]Parsons, *op. cit.*, 1924, 33 f.

[27]For sources, see *Sci. Pap. Bur. Stand.*, and recent files of *J. Opt. Soc. of Amer;* for apparatus, M. Luckiesh, *Color and Its Applications*, 1915; Köllner, *op. cit.*, and Hess, *op. cit.*

[28]For differential disks adapted to an abbreviated procedure, see Hess, *op. cit.*, 18 f.; and A. P. Weiss, A limen color mixer, this JOURNAL, 28, 1917, 408 f.

in disregard of the complications of the visual function above noted. Finally, he will be continually on the alert for the emergence of perceptual factors, or a patterning of the stimulus field suggesting perspective or 'figure and ground,' with its resultant shift of values. His records, couched in terms of hue, brightness, and chroma, should afford a new and much needed insight into the color worlds of aberrant cases, and provide a valid basis for a rating schema for use in qualifying observers for visual problems.

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METHOD IN MUSICAL PSYCHOLOGY

The pure psychology of music is far less advanced than many branches of psychology, say that of education, in theoretical validity and practical applicability. A great deal of incoördinated work has been done, but in the main it has either been approached from a philosophical, aesthetic and critical standpoint, or else in a too artificially scientific spirit which often seems to forget the real music altogether, not realizing its nature and its ramifications into the whole mental and physical life of the organism. As so often happens, the various approaches lead to the building up of mutually exclusive and incompatible systems and explanations. Indeed a psychology of music as conceived, let us say, by Dent[1] would have nothing in common with one conceived by Gatewood[2]; they would not even be based on the same types of music. I will therefore consider shortly the drawbacks to the various methods of approach or angles from which the subject has been attacked.

Musical theoreticians have consistently failed to adopt the psychological attitude and to assume the subjective viewpoint. They have discussed form and content, value, absolute *versus* program music, and the like, as objective problems, instead of recognizing that music consists essentially of emotions, images, and percepts in the minds of the composer, performer and listener. That music is 'good' which happens to appeal especially to the subjective tastes of the musicians of the period, these tastes being to a large extent determined irrationally by temperamental and various environmental conditions, by suggestion, contrasuggestion, conservatism and iconoclasticism. Any generalizations made about musical works or principles are more than likely to be contradicted by other writers, especially by those of another generation than our own, or by foreigners whose training has been unlike our own. Both the creation and the appreciation of music are functions of the personalities, or reactions to the affective and cognitive traits, of the people concerned, and if we knew all about these psychological factors and their interaction we should have a complete explanation of musical phenomena. A thorough grasp of this elementary psychological consideration would render futile most of the problems over which aestheticians and critics have wrangled for centuries.

The opposite, purely physical approach of Helmholz[3] and his English exponent, Pole,[4] is entirely off the point. All the knowledge of partials that go

[1] E. J. Dent, preface to W. Pole, *The Philosophy of Music*, republished 1925.
[2] E. L. Gatewood, An experimental study of musical enjoyment, *The Effects of Music*, ed. by M. Schoen, 1927, 78-120.
[3] H. von Helmholz, *Sensations of Tone*, 1st. ed., tr. by A. J. Ellis, 1875; 4th ed., 1912.
[4] W. Pole, *The Philosophy of Music*, republished 1925.

to make up different timbres or chords (which may often help the practising composer or performer) tells us nothing about the music that the psychological individual experiences. The notes on paper and the physical air vibrations are equally unimportant intermediaries, on an entirely different plane from the musical meanings that he imagines or perceives. The latter are not sounds, nor have they any of the attributes of musical tones, pitch, timbre, formal relations, or emotional significance apart from our consciousness of them.

The elementary psychophysical or sensationalist approach of Watt is almost as barren.[5] He would appear to regard music as a simple sensory function based on the subjective attributes or qualities of sound and fundamentally meaningless. Not only do all the objections of the Gestalt school apply to his species of 'constancy hypothesis,' but also music is one of the most obvious cases of emergence, the whole being more than the sum of the parts. Watt entirely fails to explain why music arises from and arouses emotion or aesthetic experience.

Next we may consider the researches of experimental aesthetics on musical elements, such as the investigation of the subjective effects of simple tones, intervals (bichords), or rhythms. Even these give few results that seem to be relevant to the psychology of music. Of indirect value are such results as the maintenance of characteristic attitudes towards tones in the attitude towards chords and to music itself; the effects of familiarity and repetition on the responses to unusual discords, the fallacious ascription of sadness to chords in minor keys, etc. But those who have established such data would scarcely maintain that the effects of music are a combination by association of the effects of these simple, elemental stimuli. Moreover, such effects are conditioned by the attitude of observers to musical works and to music as a whole, so that their significance is necessarily dependent on a general or synthetic view of musical psychology.

A similar objection applies to the otherwise valuable pedagogical and vocational tests that have sprung up of recent years in so striking a fashion. The psychologist often tends to delude himself in this sphere. It is wholly irrelevant to the pure psychology of music to demonstrate objectively that people who obtain a low score on a particular test are unlikely to be successful in the musical profession. The psychologist's problem is, rather, to show that the possession of certain specific traits is a distinguishing mark of the musical mind, *i.e.* to 'job analyze the musical faculty.' From this point of view tests of musical traits have been markedly unsuccessful; for the more precisely observable and scientifically measurable the trait, the less relation does it seem to bear to the musical faculty. Naturally it is desirable to proceed as far as this strictly scientific approach will take us, but correlations between simple capacities such as sense of pitch or rhythm and musicianship, though statistically significant, are so small as to have little theoretical interest. If the psychologist adopts more complex material, say, with a view to studying the mental processes involved in the perception, memorizing or reproduction of music, the objective result of his experiment or test will depend almost wholly on the musical meaningfulness of the material; in other words, on conditions determined by re-

[5]H. J. Watt, *The Psychology of Sound*, 1917; *The Foundations of Music*, 1920.

sponses to music as a whole. To one observer a musical phrase may be grammatical, logical, and easily intelligible, but another observer, whose musical experience differs quantitatively and qualitatively from that of the first, will be sure to find the same phrase of a very different degree of comprehensibility. For example, in one of my experiments, I devised a number of eight-bar tunes which seemed to me to be of roughly equal difficulty and musical logicality; nevertheless one observer took 117 seconds to learn one and 43 seconds to learn another under identical conditions, because their comprehensibility was quite different to him. The scientific experimenter will at once suggest the use of musical nonsense material in order to overcome this difficulty; *i.e.* phrases that are wholly atonal and meaningless. But even if this were possible (and contemporary music shows that its possibility is doubtful), results would immediately become as irrelevant to music as were Ebbinghaus's memory experiments to the theory of the remembering and forgetting that takes place in real life.

I can see no way out of this impasse; it will recur later in this paper. We simply have to compromise by bearing in mind both the musical and the scientific attitude. The success of modern applied psychology is due to a similar compromise; the scientific approach has been introduced, but it has been based on a fairly extensive and coherent idea of mental processes and human nature as a whole. This general idea is continually supplemented by data from psychopathology and other non-experimental spheres, so that it is wholly false to consider experimental psychology as objective from first to last. Thus intelligence tests and the like are able to aim at objective precision because they are developed out of introspective psychology. But in music the too philosophical and too scientific investigators are working comparatively blindly. Hence the view of the true province of the psychology of music which I wish to advocate is intermediate between the usual lines of approach, and it has hitherto received astonishingly little recognition.

It seems to me that a psychology of music should consist in: (1) A full description of all the factors concerned in any musical activity, particularly in its appreciation, performance, and composition, obtained both by objective observation and by introspection. (2) Experimental proofs of the constancy, relevancy, and significance of such factors. (3) A classification and generalization of these results in terms of recognized psychological principles. Naturally all three categories should develop together, but owing to the inadequacy of (1) and (2) this has scarcely been attempted. Only on such a basis can the more usual approaches be theoretically justified, namely: (4) the philosophical methods, (5) the scientific methods.

Admittedly the performer and the composer constitute a very difficult problem; they are far less available than the listener to the psychologist, since amateurs in these fields are so largely concerned with rather uninteresting technical details. I have, however, suggested a fruitful approach to the investigation of musical composition; namely, a study of great composers by means of their biographies.[6] The data, are of course, unreliable; composers usually dislike introspection in that it tends to spoil inspiration. I obtained, nevertheless, some useful results of a quasi-objective nature from a comparison

[6] P. E. Vernon, The personality of the composer, to be published shortly in *Music and Letters*, 10-11, 1929-1930.

of their personalities, the influence of sociological factors, their methods of work, etc., and the consequent characteristics of their music.

But clearly it is the listener who should claim our first attention. The nature of varieties of musical appreciation should be the initial and central problem. Very few investigators have given any account as yet of what listeners, other than themselves, actually think, feel, and do; or have tried to take into consideration all the factors, auditory and non-auditory, that enter into musical situations.

This direct approach is fraught with difficulties and dangers, so that reasons for its previous neglect are not hard to find.[7] They may be enumerated as follows.

(a) Many of the experiences in listening to music are auditory or kinaesthetic and organic, and cannot easily be put into verbal terms.

(b) Time is such an essential constituent of the experience that ordinary retrospection with regard to mental processes of short duration is inapplicable. Many people tend to become absorbed in music to which they are listening, so that, more than in most branches of psychology, introspection may destroy its own data.

(c) Few musical people have psychological training. In my own research, however, I found that they were better at introspection than I had expected, since they seldom listen in a completely passive and subjective fashion, but remain critical of what they hear and of their reactions to it.

(d) Every hearing of a musical item is essentially unique, depending largely on the degree of familiarity with the work, the mood of the moment and other extramusical factors. Introspections, however good, have little valid application to other listeners, to other pieces of music, or even to the same music at another hearing.

(e) The chief difficulty is produced by what one may call the musical psychologist's fallacy, so frequently does it intrude, namely, that of assigning his own modes of thought and attitudes to his observers. If he is musical himself he tends to regard the musically untrained in a lump as inferior copies of himself, and is quite unable to comprehend the individuality and differences of their points of view. If, as seems to be more often the case, he is not really musical himself, he shows even less comprehension of the musician's point of view, often neglects him altogether, or considers his somewhat intellectual type of aesthetic appreciation as anomalous to musical psychology. Again, few workers have employed music that is accepted by the musician as really good or capable of producing genuine aesthetic experience.

It may be objected to my notion that both the experimenter and some of his observers should be drawn from the ranks of well trained and highly discriminating musicians that such people constitute less than 1% of the music-loving public. Yet the following facts must be acknowledged. (1) They are the only people who approximate to understanding what they hear; they therefore have the best right to speak about it. (2) In spite of the distortions exerted by fashion, suggestion and other affective factors, it is the trained (*e.g.*

[7] It should be pointed out that Vernon Lee is the one investigator who has realized this attitude (cf. Varieties of musical experience, *North Amer. Rev.*, 207, 1918, 748-757).

composers, conductors, and critics) who chiefly determine the music that shall last; hence they are the most worthy of consideration. (3) I obtained good evidence that the musically untrained tend to incline to the attitude and opinions of the trained towards music, the more their experience.[8]

Naturally the ordinary music lover should not be neglected: he throws as much light on the highly developed product as child psychology does, say, on language. But that is no reason for the apparent divorce of music from the musician by most experimenters in this field. Similarly, the pure musician who entirely lacks the psychological outlook would be of no use in investigating these problems. On both sides we need to find the happy man.

Examples of this psychological fallacy may be cited. Several classifications of types of musical appreciation have been advanced, but from lack of analysis and comprehension of other listeners they are most unsatisfactory. Some investigators group their observers into those who listen for rhythm, melody, or harmony. In my own work I have not discovered a scrap of evidence for this: the investigators are merely putting their own theoretical predispositions into the mouths of the observers. Again, the majority of writers describe the emotional or subjective and the intellectual or objective types, but entirely fail to recognize the very large and important class of those who synthesize the two attitudes in their highest appreciation. It seems to me that the truest aesthetic appreciation is essentially a fusion of emotion and intellect; it is an appeal to all sides of human nature, not to one isolated set of impulses (though I am quite prepared to admit that this is my personal prejudice or predisposition, and that it does not really apply to others).[9] From my own investigations I think it probable that the purely subjective listeners are usually, though not always, the wholly unmusical who can only interpret what they hear in emotional terms, while the purely objective are either desiccated academic critics or else amateur musicians who are trying to learn to comprehend and appreciate by reacting against excess emotion; the majority of trained musicians grow out of these primitive stages and harmonize the two attitudes.

The fact is that in dealing with an intimate, subjective experience like musical appreciation it is intrinsically impossible for observers and experimenters really to understand one another. Let us free ourselves from the fallacy that words are universal, that they mean the same to everybody. Test instructions, aspects or traits to be rated, etc., mean quite different things to different people. For instance at an experimental concert that I conducted the audience were asked to rate, among other categories, the amount of aesthetic appeal or value. As far as I could make out, some took this to mean:—"the feeling of beauty or aesthetic pleasure produced in me;" others to mean: "the value of the music, quite apart from whether I enjoyed it or not;" and still others to mean: "the amount of thought and criticism I gave to its value;" and so on.

[8]It is well known that even quite unmusical people tend to prefer good music in the long run, and an experimental demonstration of this has been given by A. R. Gilliland and H. T. Moore, The immediate and long-time effects of classical and popular phonograph selections; essay in *The Effects of Music*, ed. by M. Schoen, 1927, 211-222.

[9]Experimental evidence for this is given in my article, Non-musical factors in the appreciation of music, *Musical Times*, Feb.-April, 1929, 123-124, 227-228, 320-321.

It seems to me that all investigations which try to express purely qualitative, subjective states in objective, quantitative terms are doomed to failure. The aesthetic experience is, almost by definition, a new integration of experiences; it is the least stereotyped, the least objectifiable of mental processes. When we are dealing with extramusical factors such as over-emotional or over-intellectual responses, visualization, gregarious influences, synaesthesis, and the like, we have some more or less definite, stereotyped processes that can be observed and described fairly well in scientific terms. But when we get to these essentially personal experiences we find, first, that they cannot be translated adequately into words, and secondly, that communication is impossible, since the experimenter cannot comprehend the mental organization of observers who differ widely from him. If he tries to describe others he is really only expressing varieties of his own mental organization, his own ideas as to these attitudes and experiences of others. He may, of course, quote their statements freely, but the chances are that he has selected the results that he presents, or that he has biased the observers by setting the situation in which they introspect. Hence, in respect to this central aesthetic aspect of musical appreciation, these considerations indicate further difficulties to be guarded against in experimental attacks.

Two extremes of experimental method have been exploited by different investigators, the free or uncontrolled and the scientific or controlled. In the former anything and everything the observer has to say is recorded either by the observer himself or by the experimenter. This inevitably means that the observer rationalizes, selects, and abstracts so that only partial bits are displayed to the view of the experimenter. Important points are often neglected, sometimes because, like synaesthetic responses, they seem so obvious to the individual concerned. There is also a tendency, I find, for observers not well trained in introspection to record mere artificial criticisms of the music rather than what they actually thought and felt. On the investigator's side, also, the more phenomenological the introspections the less is it possible for him to be impartial in interpreting and working up the results, quite apart from the already mentioned difficulties of communication. Again, I have found it most difficult to isolate or obtain definite evidence on any point, using this uncontrolled method. For instance, at an experimental concert where it was applied one or two of the items received a rather inadequate performance, and I had hoped to trace the effects of this in my observers' introspections. Actually very few mentioned it, but for all I know this may have been because, (1) they failed to notice it at all, (2) they were dissatisfied but failed to analyze their displeasure down to the performers, (3) they noticed the technical faults but forgot about them before they had time to write them down, (4) they disliked recording their criticisms for various reasons such as politeness, fear of making blunders themselves, etc. In fact, so many factors influence the introspections that without some form of control it is almost hopeless to expect to extract any reliable data, to introduce any generalization or systematization.

The fully controlled method can, however, be even more unsatisfactory. Usually, to obtain a degree of objectivity, specific questions have to be answered by the observers (sometimes only 'Yes' and 'No' answers being permitted), or else specific emotions or aspects of the music have to be rated for

each musical item. The inevitable result is that the observer's thoughts are forced into the experimenter's grooves, for these abstractions only apply to the mental processes of those who devise them; only if the experimenter could comprehend and allow for the individual differences of all his observers are such methods really justifiable. In general, the better controlled and systematized the conditions, the more is the situation likely to become abnormal and artificial. Many experimenters have taken observers singly, not realizing that for the great majority of listeners social influences and gregarious factors play a very large part in appreciation. On the other hand if several observers approximating to a normal audience, listen simultaneously, they will have to write their own notes, and a further unnatural factor is likely to be thereby introduced, that of long, possibly idle intervals between the items. For similar reasons the elimination of the personality of the performers by the use of the phonograph is unsound; almost every investigator except Gilman has employed phonographically reproduced music so as to standardize the stimuli,[10] and that at a period when it even less resembled the original than nowadays. We have got so used to the vibrations and distortions of tone produced even by good phonographs that we scarcely notice them; yet in so far as good classical music and the like depend largely on exact and balanced performances, they cannot but make appreciation more difficult. Very little general observation is needed to realize that, for most people, music must be performed "in the flesh" for normal appreciation.

Other experimenters have made attempts to eliminate or control some of the extramusical factors that influence normal musical appreciation; for instance, they have not told to the observers the names of the musical items or composers. If they think that thereby they can make observers appreciate the music solely for itself, unbiased by technical interests and the like, they must be singularly ignorant of the average listener's mentality. The individual who listens to music as pure sounds is a myth based on our psychological theories; his perception of what he hears inevitably depends on his previous experiences with music, his already developed perceptual systems, his tendencies towards intellectual analysis or emotional interpretations. In two experimental concerts of my own I combined a rating method with uncontrolled introspection: in one programs were provided, the performers were in full view, etc., but in the other the music was not announced and the performers were behind a curtain.[11] In this second, abnormal concert not only was there far more technical interest displayed in the listeners' introspections (88% kept on trying to guess the identity of the composers and the music in spite of being asked not to do so), but also the technical interest was more distorted and inappropriate than when, as in the first concert, it was guided normally by a program. The guesses were astonishingly incorrect, the ratings and remarks were far more vague and difficult to interpret. The more musical people are incapable of listening normally until they have pigeonholed what they hear technically and historically; those, on the other hand, who habitually

[10] B. I. Gilman, An experimental test of musical expressiveness, this JOURNAL, 4, 1892, 558-576; 5, 1893, 42-73.
[11] Cf. P. E. Vernon, op. cit.

visualize or interpret what they hear emotionally continue to do so even without titles to guide them.

These observations show how difficult is any approach to musical psychology. There is, in fact, a fundamental opposition between aesthetic experience of any kind and scientific method, which no investigation can fully overcome. The study must be, in its very nature, prescientific, for the present, and we have to compromise. Naturally a scientific approach should always be introduced where feasible, but the artistic attitude must not be forgotten, else the investigator will find that he has destroyed the phenomena he was trying to investigate, and that his results are quite irrelevant to music or art. We must not scorn to make use of 'anecdotal' and other heterodox methods, general observations of the behavior and characteristics of audiences, conversations and expressions of opinion on musical problems by people of all degrees of musical experience, questionnaires, books on music and musical criticisms, etc. In this way the investigator may progressively accumulate a better comprehension of other listeners' points of view, and so be in a better position to apply enlightened scientific methods.

The enormous advances made by experimental psychology are due not merely to its absorption of scientific aims and principles, but also to the thousands of years of 'arm-chair' consideration which binds together and introduces some coherence into these more detailed and objective approaches. The psychology of music can never attain to a similar status until it develops a like general theoretical background that accords with the musician's as well as the psychologists's outlook.

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A CRITICISM OF THE CONFIGURATIONIST'S INTERPRETATION OF 'STRUCTURALISM'

For some time a point of view has been developing that goes by the name of Gestalt Psychology, a construction taking its proximate rise in researches devoted to the phenomena of perception. This school has been active in systematizing the materials derived from the province of psychology as a whole. Not only have the configurationists produced a large number of researches, many of them possessing a high order of merit, but they have also reinterpreted the experimental findings of other schools in terms of Gestalt-Theorie.

Without question, this new point of view has stimulated psychological investigation at large. If the positive contributions of the configurationists have not always seemed to fulfil the expectations awakened by their sanguine promises, at least the criticisms advanced by them against the older and more current interpretations have been most suggestive, even though they have not always been entirely fair.

Discontent with the older structuralism had been growing long before the Gestalt psychology arrived upon the scene. Within the ranks of the structuralists themselves Külpe, at the beginning of the century, broke away in part from the teachings of his master Wundt and founded a new mode of attack upon the higher thought processes. In the Würzburg monographs

there is more than ample indication of the fact that Külpe and his associates considered the Leipzig dogma in need of readjustment and supplementation. With an historical background of steadily increasing doubt as to the adequacy of classical structuralism, Gestalt psychology emerged and announced itself as the 'New Psychology.'

Configuration utilizes the method of functional analysis and is a system built upon the doctrine of wholes. It flatly rejects the structuralist's plurality of elements and regards askance the whole program of the structuralist's analytical approach. The configurationist sees everywhere wholes, uniques, that are not to be resolved very far, if at all, into constituent parts. In brief, Gestalt psychology deals with gross meanings.

The present discussion is not at all concerned with detailed consideration of the positive contributions of the Gestalt school. Rather, the question is here raised whether the violent attacks launched by this school against the psychologies called 'structural' might not possibly be modified, toned down, without any loss of dignity to Gestalt psychology and with a consequent increase in fairness toward structuralism.

An unfortunate tendency is apparent on the part of the configurationists to portray structuralism in such fashion as to set it up to unreserved ridicule. The advocates of Gestalt let slip no opportunity to caricature the teachings of structuralism. Such caricaturing would seem to have become almost an obsession with certain exponents of Gestalt-Theorie. A survey of their polemical writings might well lead the uninitiated to conclude that the configurationists were the first to grant any consideration to wholes, uniques, emergents, although, to be sure, Mach and Von Ehrenfels are given some acknowledgment by them. Also, according to the account given by the Gestalt writers, this school was the first to demonstrate the lack of a point to point relationship between stimulus and sense-perception.

Let us enumerate a few typical criticisms formulated by the configurationists against structuralism. The term attention, for instance, should be relegated to the scrap-heap and be replaced by the term attitude. The term integration has no proper place in psychology, for it is necessarily implicative of "Und-Verbindung." The phrase "due to experience" should be tabooed: instead, say "due to the configuration." Again, it is not a matter of utilizing both the concept of figure-ground and the concept of clearness-levels, but the latter is to be excluded rigidly from all respectable psychological discussion. As for the problem of learning, the term "Insight" solves most of the difficulties. It might be suggested here that the term insight functions as a most convenient "maid-of-all-work" for the configurationists. When search is made for a definition of insight, it at once appears that insight may assume so many forms, is so distinctly protean, that one is sometimes forced to wonder at the magical powers attached to this word.

The configurationist impresses one by his so frequently strained attempts to appear thoroughly *new*. A quotation from Koffka will do excellent service in this connection; he says, "At the end of her twenty-first month, the names of forms which the child had learned from a toy consisting of small and variously shaped pieces of cardboard, were for the first time applied to things in her

environment. Thus, for instance, the folded edge of a man's collar was called a triangle. This should not be understood to mean that the collar edge simply reproduced the name *triangle* on account of its similarity with the triangular cardboard, but rather that the configuration which was acquired in the use of the toy now entered into the perception of a man's collar."[1]

The statement that "the configuration which was acquired in the use of the toy now entered into the perception of a man's collar" is intended to be illuminating and immediately to clear the whole complicated problem. All is explained by the invocation of "configurational law"! Incidentally this sort of explanation is supposed to demonstrate the failure of the older psychology.

If one happens, for example, to be concerned with the problem of the visual perception of depth, he should not, according to the Gestalt psychologists, be so old-fashioned as to recognize that anything of explanatory value may be derived from the facts concerning the disparity of retinal images, but he should sweepingly assert that the phenomenon is "due to a conflict of configurations." When pressed for a more specific answer, he should further maintain that the events under consideration are *primarily* central. That is to say, one should promptly place the problem in a region about which practically nothing is known as to particulars. This type of explanation gives one a sense of security as it cannot be disproved. Unfortunately, however, this sort of procedure resembles nothing so much as the course adopted by a speculative philosophy. Still one may, if he is pushed further or is in doubt, take refuge behind the word "maturation"; where he is safe.

The Gestalt psychologists have all too frequently been guilty of setting up theoretical straw men and then knocking them down. One thoroughly unfortunate result of this wholesale and destructive attack upon structuralism takes the form of a dulling of the critical sense of students. Take for example Koffka's "*Growth of the Mind.*"[2] In this work a devastating onslaught is made upon structuralism. The young student, after a reading of the book, is apt to make up his mind that no good whatever is to be found in structuralism, and that Gestalt psychology either has solved or is well advanced on the high road toward the solution of all problems.

In order to show that the structuralists have not been entirely blind to the fact of unique wholes, of Gestalten, quotations will now be made from the writings of Wundt, Külpe, and Titchener.

Wundt has said: "the expressions outer experience and inner experience do not indicate different objects, but different points of view from which we take up the consideration and scientific treatment of a *unitary* experience."[3] Again: "The immediate contents of experience which constitute the subject-matter of psychology, are in all cases processes of a composite character. Sense perceptions of external objects, memories of such sense perceptions, feelings, emotions, and volitional acts, are not only continually united in the most various ways, but each of these processes is itself a more or less composite

[1] K. Koffka, *The Growth of the Mind*, (R. H. Ogden, tr.) 1924, 289-290.
[2] *Op. cit.*
[3] W. Wundt, *Outlines of Psychology* (C. H. Judd tr.), 1907, 2 (italics in this and the following quotations are mine).

whole."[4] He furthermore states that "all the contents of psychical experience are of a composite character. It follows, therefore, that psychical elements, or the absolutely simple and irreducible components of psychical phenomena, are the products of *analysis and abstraction.*"[5]

Külpe has stated: "But there is one other point to which attention must be called if we are to avoid misunderstanding of the character of the conscious elements. . . . By help of the attention we may, it is true, subject even the less intensive elements to special investigation or observation; but *real isolation,*—the actual experience of one single sensation, for instance,—*can never take place.*"[6]

Titchener says: "Whether we begin our psychology with 'sensation' or with a case of 'association of ideas,' we are always beginning with an abstraction."[7] He goes on to say: "This sensation, which is a structural unit of the adult mind, is not the genetic unit of mind at large. Mind has not grown by aggregation of sensations, by the simple addition of our 'blue' and 'yellow' to a given 'black' and 'white' that are also like ours, and by the further addition, still later, of a ready-made 'red' and 'green.' Even granted that we could analyse the primitive mind into sensations, still its 'black' and 'white' would be so different from our own as to be hardly recognisable. At the time when the heart begins to pulsate, there are no muscle fibrillae in the myocardium; we have the sight of purely protoplasmic, undifferentiated cells making strong rhythmical contractions. If, now, the structural elements of the primitive mind are 'sensations,' they are sensations only in the sense that these primitive heart-cells are 'muscle' cells. . . . . Knowing . . . does not come, any more than does mind, from the addition of sensation to sensation."[8] Titchener maintains: "This fact, then, is important: that we may attend to the *perception as a whole,* . . . But the other fact, that systematic observation always reveals the complexity of the perception, is of no less importance."[9] Further on he states:"Let us assume for the moment that any perception may be analysed, without remainder, into a number of sensations. It would still be true that the mere enumeration of these sensations is not an adequate account of the perception. For the sensations which we find, in the particular case, form a group; they have been selected, singled out, marked off, from the other contents of consciousness."[10] "Perceptions are selected groups of sensations, in which images are incorporated as an integral part of the whole process. But that is not all: the essential thing about them has still to be named: and it is this,—that perceptions have meaning. All perceptions mean; they go on, also, in various attributive ways; but they go on meaningly."[11] "We have no reasons to believe that mind began with meaningless sensations, and progressed to meaningful perceptions. On the contrary, we must suppose that mind was meaningful from the very outset."[12]

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[4]*Ibid.*, 28.
[5]*Ibid.*, 31.
[6]O. Külpe, *Outlines of Psychology,* (E. B. Titchener tr.), 1908, 21.
[7]E. B. Titchener, *Experimental Psychology,* Vol. I, Pt. ii, 1901, 3.
[8]*Ibid.*, 4.
[9]*Text-Book of Psychology,* 1910, 349.
[10]*Ibid.*, 364. [11]*Ibid.*, 367. [12]*Ibid.*, 369.

Titchener, discussing James on the problem of analysis, makes the following significant statement: "A third inference, which is certainly as dangerous as the two mentioned" [by James] "is this: that the psychological elements, just because they are elementary, are chronologically the first things in mind, so that perceptions grow, are formed, by the interconnection of originally separate sensations. The elements are, as we have seen, the results of analysis; the perceptions are the original things, and the sensations are found in them by observation; perceptions are given us, and we discover that they are analysable. Misunderstanding here is fatal to the student of psychology, for it means misapprehension of the central psychological problem."[13]

This last quotation in particular is a most important one for the correct understanding of the structuralist position. Gestalt psychologists have been ranting about the chaos theory of mind presumably propounded by the structuralists in the field of genetic psychology. Have they ever paused in their criticisms to read the passages quoted above?

Analysis of the 'structural' point of view reveals three principal facts. First, that clear and unequivocal recognition is accorded the phenomenal actuality of wholes, *Gestalten*. Second, that careful distinction is drawn between the systematic 'sensation' and the observational attribute. Third, that sensation is treated as an *analytic, not as a genetic*, concept. Titchener, in the last quotation above given, states it to be a necessary supposition that mind was meaningful from the very first.

It would appear that configurationists have not noted these facts. Spearman, writing of Gestalt psychology, says, "if this imposing movement is to be traced back to any single source, the best claim is undoubtedly on behalf of Wundt with his well-known principle of 'creative synthesis.'"[14] Boring recently asked, "Is Gestalt a protest against Wundt and his influence? What about Wundt's *Aktualitätstheorie*, his creative synthesis and the law of psychic resultants? Was Wundt nearly so atomistic as his critics would have him be, or did he too have a vision similar to that of James, and Ehrenfels, and Wertheimer'?"[15]

The sensory attribute is, for the structuralists, the ultimate psychological datum. The systematic 'sensation,' however, is for them *always a logical abstraction*. In the utilization of abstractions psychology differs in no wise from other disciplines, such as physics, with its concepts of an ether and quanta.

No doubt the term structural has been an unfortunate one, leading as it so frequently has to an interpretation of the word as synonymous with the term atomistic, and 'sensation' as synonymous with 'psychic atom.' Titchener was aware of the unfortunate misuse so often made of the word structuralism, and on account of such misuse finally expressed his view that the terms structural and functional were obsolete as applied to psychological systems.[16] The word

[13] *Ibid.*, 350-351.

[14] C. Spearman, The new psychology of 'shape,' *Brit. J. Psychol.*, 15, 1925, 212.

[15] E. G. Boring, The problem of originality in science, this JOURNAL, 39, 1928, 88.

[16] E. B. Titchener, Functional psychology and the psychology of act, this JOURNAL, 32, 1921, 533.

structure has been historically weighted in the direction of materialistic meanings of one order or another, and is applicable to dynamic patterns such as constitute the subject matter of psychology in an analogical sense only; and such analogical application of this term is the only one that the structuralists would admit.

As a matter of fact, the structuralists acknowledge the phenomenal priority of wholes, but are alive to the fact that due to attentional limitations, an exhaustive account of the contents of a given whole can be obtained only under the condition that the various aspects are selected and described one by one, and moreover that a description of stimulus conditions does not constitute a description of the psychological order.

The Gestalt psychologists have had much to say concerning the glaring defects of the 'structural' mode of analysis and the effectiveness of the so-called functional, indirect, analysis. But, as concerns the supposedly artificial nature of the structural analyses, it seems that such analyses can not be so completely absurd as the configurationists have represented them to be in view of the fact that certain synthetic checks on the attributive analyses have actually been made. There is, for instance, the synthetic experiment in tactual perception, where such perceptions as those of liquidity,[17] oiliness,[18] and stickiness,[19] have been artificially produced upon the basis of attributive analyses without the utilization of liquid, oily, or sticky stimuli.

The concept of wholes is of the greatest importance for science in the large, but it can be done to death, it can be utilized as a deus ex machina. More than half a century ago Wundt wrote confidently that the faculty psychology had received its deathblow. We know, however, that although the faculty psychology in its grosser form has vanished, it nevertheless persists under the guise of various subtle forms. Occasionally, one might be willing to venture the appellation Neo-Faculty Psychology in respect to configurationism. Its manipulation of entities called *Gestalten* has frequently all the appearances of faculty psychology on masquerade.

The problem of formal properties is the one treated by the Gestalt psychology. But there is another and equally important problem. This is the problem of filling, of content; and the problem of content has been most thoroughly dealt with by the psychologies called structural.

As concerns Wundt, he has held that everything comes to the immediately experiencing subject in wholes. These require, however, according to the Wundtian school and its modifications, constitutive analyses into attributive terms. Wundt and his disciples have never taught that mind is the result of the bundling together of psychic atoms called sensations, even though the configurationists glibly refer in terms of seeming contempt to the "bundle-hypothesis" of structuralism. To the structuralist, mind is meaningful from the very outset. Wundt is not an atomist, whatever else he may be dubbed, and what-

[17] I. M. Bentley, The synthetic experiment, this JOURNAL, 11, 1900, 405.
[18] L. W. Cobby, and A. H. Sullivan, An experimental study of the perception of oiliness, *ibid.*, 33, 1922, 121.
[19] M. J. Zigler, An experimental study of the perception of stickiness, *ibid.*, 34, 1923, 73.

ever his failings may have been. If the contents of psychological pattern have ever received highly technical and exhaustive description from the point of view of constitution, then Wundt and his followers deserve the credit.

Bear in mind, along with the attractive aspects of Gestalt psychology, the existence of many internal bickerings within its own domains. Instead of talking about *the* Gestalt psychology it would be more appropriate to talk about the Gestalt psychologies. *Gestalt-Theorie* is just that: a theory, an hypothesis, and a valuable one. But it is not a revelation of infallible order.

It is the conviction on the part of many that the well-known lines written by James forty years ago at the conclusion of his *Principles* are quite applicable to the condition of the psychologies and the near-psychologies of to-day. Recognition must be more fully accorded by some thinkers to the fact that mental organization may be described and interpreted from a number of points of view and in terms of different observational attitudes. No single point of view can be deemed exhaustive of the possibilities of description. In view of the immense complexity of mental structure-function, and in consideration of the marked temperamental differences existing between workers in the field of psychology, it would be well to maintain an open-minded, appreciative, but carefully critical attitude toward a number of systems. Omniscience can not be attributed to a single system. Each system has had something of value to contribute to our total fund of knowledge. The psychologist above all investigators should strive to develop and maintain a balanced historical sense. Only then can a system of psychology, be it 'structuralism' or the still adolescent *Gestalt-Theorie*, obtain a stable growth, effective application, and just evaluation.

Clinton, N. Y. PAUL CHATHAM SQUIRES

SEX-DIFFERENCES: SOME SOURCES OF CONFUSION AND ERROR

The traditional point of view toward the mental ability of women is being modified rapidly. The average man appears to have changed his stock of generalizations and rationalizations to some degree. Popular magazines now carry articles which attempt to place the new knowledge vouchsafed by 'science' before the multitude. The shrewd publisher finds a ready market for 'scientific' books which deal with this alluring topic. The novel and short story also take their turn in providing half-truths for the 'lay reader.' The total effect of such writing has brought about a decided change in attitude, but whether or not the information so disseminated has scientific validity is a question of importance and doubt. The writers of this article feel that sweeping dogmatic attitudes toward all problems of sex are almost always based upon decidedly scant information. Magazines, novels, *et al.*, have disseminated much information that is clearly unsound or unproven.

False conclusions with reference to sex-differences have been due in many instances to one or more of the following influences: (*1*) differential selection of test items; (*2*) differential selection of the individuals studied; (*3*) the limited number of individuals studied; (*4*) inaccurate, or crude, statistical procedure and treatment of results; (*5*) hasty generalization due to the assumption that findings are applicable to all age-levels when certain age-levels only have been

studied; (6) hasty generalization due to careless use of terms; and (7) the assumption that group differences are innate or inherited predispositions.

The above influences will be commented upon briefly in the discussion following.

(1) *Differential selection of test items.* Girls have been found to excel boys in linguistic ability by practically all investigators. Conversely, a consistent difference in favor of the boys has been found on practically all tests involving the use of number and in most tests which demand ability to manipulate mechanical contrivances. Inclusion of many items of the above types of questions in intelligence tests provides therefore a discriminating factor in test results.

The superiority of men over women on the results of the Army Alpha Test is well known. Although a few investigators at first thought that the results were indicative of male superiority, practically everyone now admits that this particular test was weighted heavily in favor of men.

In contrast to the Army Alpha Test is the 1928 edition of the Scholastic Aptitude Tests used by the College Entrance Examination Board.[1] In this test the numerical sub-tests, *e.g.* the test in arithmetical problems and the one in number-series completion, were deleted (because of statistical studies of preceding years) and sub-tests employing synonyms, easy paragraph reading, hard paragraph reading, classification, antonyms, analogies, and double definition were used. On the modified test the girls exceeded the boys by 17.28 points. The mean for the boys was 490.86; for the girls, 508.14.[2]

The above citations explain why intelligence test data are often not convincing when used for making sex-comparisons. Nevertheless, most of the studies in which intelligence tests have been employed reveal small differences only. A common practice in dealing with intelligence tests is to consider differences in total or composite performance upon a number of tasks calling for widely varying mental reactions. The expression of the total performance is usually in terms of M.A. or I.Q., blanket terms including many varied and totally unlike cognitive responses. Terman finds that girls do slightly better in the Stanford-Binet Test than do boys of the same C.A. until the age of thirteen is reached. Miss Goodenough believes that the slight superiority of girls is due to the fact that "more than half of the total number of items in the section of the Binet under consideration are chiefly upon types of performance in which females have been found to exceed males at all ages for which information is available, while the types of performance in which males have usually been found to exceed females are practically unrepresented."[3] Because of the conspicuous differences in the types of test items employed in the intelligence test, "the question of mental precocity as related to sex must be answered in terms of specific functions or traits, rather than in terms of unanalyzed general tendencies." It appears therefore that little data can be adduced to demon-

[1]The tests employed by the College Entrance Examination Board are called Scholastic Aptitude Tests not because they differ fundamentally from the so-called intelligence tests, but rather because it is now recognized by many that the earlier descriptive term was inappropriate.

[2]C. C. Brigham, *Third Annual Report of the Chairman of the Commission on Scholastic Aptitude Tests*, 1928.

[3]F. L. Goodenough, The consistency of sex differences in mental traits at various ages, *Psychol. Rev.*, 34, 1927, 458 f.

strate male superiority in general intelligence; reported differences have often been due largely to the weighting of the test-instrument in favor of one sex or the other.

(2) *Differential selection of the individuals studied.* In 1911 Cohn and Dieffenbacher tested students in one of the few co-educational schools of secondary school rank in Germany.[4] They found the girls superior to the boys. Since these German scholars did not believe the female to be superior to the male, they attributed their results to the fact that the girls comprised a more highly selected group than the boys.

In America several investigators have found high school senior boys to be superior to high school senior girls in intelligence test-scores.[5] Such findings have been explained usually as due to the more rigorous selection of boys throughout the elementary and secondary school years. It is of course well known that boys are eliminated from school more rapidly than girls and that dull pupils tend to drop out of school at earlier ages than bright ones. Since sex-difference in rate of elimination from school occurs at very early ages, it is only logical to conclude that difference in intelligence test scores at the high school level may be due to differential selection of the individuals studied.

In the above-mentioned studies the fact of differential selection was taken into account in the interpretation of findings. Nevertheless, differential selection is often overlooked or neglected in other studies which report sex-differences in traits other than intelligence.

(3) *The limited number of individuals studied.* The necessity of having an adequate sampling of subjects is obvious, but it is often ignored. A very large number of studies are based upon responses of less than a dozen subjects of either sex. Winsor reports that differences between five men and five women have been cited as evidence of greater variability of the male.[6] One writer studied only two subjects, himself and his wife; he then wrote an article on sex-differences.[7] When conclusions based upon studies which have used only a few subjects are given a second- or third-hand recital, (and this is frequently done) the number of subjects studied is often not mentioned and the reader is then given an erroneous impression regarding the reliability of the original studies.

(4) *Inaccurate or crude statistical procedure.* Winsor has shown that reported sex-differences in variability are due often to peculiarities of formula for determining variability.[8] Measures of gross variability occasionally have displayed one result and measures of relative variability have revealed a quite

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[4] J. Cohn and J. Dieffenbacher, *Untersuchungen über Geschlechts- und Begabungsunterschiede bei Schülern*, 1911. Quoted by H. T. Woolley, The psychology of sex, *Psychol. Bull.*, 11, 1914, 375.

[5] W. F. Book, *The Intelligence of High School Seniors*, 1922; S. S. Colvin and A. H. MacPhail, Intelligence of seniors in the high schools of Massachusetts, *Bull. U. S. Bureau of Educ.*, No. 9, 1924; D. G. Peterson and T. A. Langlie, The influence of sex on scholarship ratings, *Educ. Adm. and Sup.*, 12, 1926, 458-469.

[6] A. L. Winsor, The relative variability of boys and girls, *J. Educ. Psychol.*, 18, 1927, 327.

[7] H. D. Marsh, Individual and sex-differences brought out by fasting, *Psychol. Rev.*, 23, 1916, 437-445.

[8] *Op. cit.*

different tendency. The careful student should demand therefore the formula which has been employed for measuring variability when sex-differences are reported.

When individuals have been grouped on the basis of sex, the resultant group differences have usually been regarded as 'sex-differences.' L. S. Hollingworth points out that a chance difference would be found if the individuals in certain groups were re-segregated on the basis of any incidental factor—say, eye-color or presence or absence of physical anomalies—and the same comparisons made.[9] Apparent sex differences must be examined always in terms of probable error.

For many years, workers assumed that the female was inferior to the male in general intelligence. A convincing part of the evidence was the difference in brain-weight between male and female. Today it is realized that brain-weight is an unreliable measure of intelligence. Woolley states (and practically everyone now admits) that the relatively small weight of the female brain as compared with that of the male is of little significance in affecting mental ability.[10]

Prior to the past dozen years it was commonly assumed that the sex-impulse was much less intense in women than in men. This drive has been studied extensively in animals. In certain species it has not been found that female animals show a weaker sexual urge than males. Females surmount obstacles and traverse electric grids in order to reach their mates as often as do the males. Moreover, among the animals studied in the laboratory, the amount of activity directed toward a member of the opposite sex does not appear to be greater in the male than in the female. Modern students are beginning therefore to doubt the existence of inherent sex-differences with reference to the strength of the sex-propensity in man. They are inclined to attribute apparent difference to such factors as social custom, community mores, etc. This attitude has developed almost entirely within the past decade and it has resulted largely from improved methods of animal experimentation.

(*5*) *The assumption that findings are applicable to other age-levels than those actually studied.* A fifth source of error is hasty generalization due to the inclusion of all or nearly all age-levels in the formulation of conclusions when certain age-levels only have been studied. Thurstone has found that scores vary markedly from grade to grade and from age to age.[11] Dunlap also has pointed to the fact that variability depends (among other things) on age-level.[12] Because of the irregular development of numerous human characteristics, one may expect to find that at a certain age-level girls will be more variable than boys in some regards, and at an earlier or later age the reverse may be true. With respect to any characteristic, the greater variability will occur during the period in which the most rapid changes in that characteristic are taking place. The averages for total behavior manifestation (covering several years) sometimes obscure the real sex-differences, and in other instances

[9]L. S. Hollingworth, Comparison of the sexes in mental traits, *Psychol. Bull.*, 15, 1918, 427-432.

[10]H. T. Woolley, A review of the recent literature on the psychology of sex, *Psychol. Bull.*, 7, 1910, 335.

[11]L. L. Thurstone, A method of scaling psychological and educational tests, *J. Educ. Psychol.*, 16, 1925, 433-451.

[12]K. Dunlap, *Social Psychology*, 1925, 32.

the averages exaggerate the general trend. The writers are here interested in emphasizing the fact that the findings for one age-level do not hold invariably for all other age-levels. They wish also to call attention to the danger of obscuring significant differences by treating *en masse* data secured from heterogeneous age- or grade-groups. These points have frequently been neglected or minimized when findings with reference to sex-differences have been reported.

(6) *Careless use of terms.* Psychological literature contains much material on sex-differences referring to such traits as "fidelity of report," "ability to describe an object," "range of information," etc. Early writers seem to have considered 'fidelity of report' a unitary trait; they certainly assumed that simple devices measured complex traits and that a single study of a few subjects justified broad generalizations.

In discussing sex-differences with reference to "fidelity of report" Whipple writes: "In all of Stern's work, both in narratives and depositions, with pictures, or events, or estimations of times and distances, whether under oath or not, the reports of men have been more accurate (by from 20 to 30%), ... Stern's conclusions, however, have not been confirmed by Wreschner, Breukink, or Miss Borst."[13]

When sex-differences are reported in vague and unanalyzed terms it is of course inevitable that the findings will be contradictory and the conclusions conflicting. McGeoch has demonstrated in three recent articles that the ability of children from ages 9 to 14 to make a correct report is specific in relation to the material reported.[14] It is therefore ambiguous to discuss sex-differences in 'fidelity of report' unless the specific material to be reported and the specific test-conditions are set forth.

A second example of the use of terms that are too inclusive is afforded by the literature dealing with 'sensitivity to pain.' Many investigators have reported that women are *more* sensitive to pain than are men. Ottolenghi and Lombroso state that women are markedly *less* sensitive than men to electrical stimuli.[15] It is evident of course that pain may be aroused in innumerable ways, and that greater sensitivity to pain aroused by one method does not posit greater sensitivity to every kind of pain.

A more modern example of the careless use of terms is the statement that females show a consistent superiority over males in "verbal memory." The term 'memory' connotes *a very wide variety* of performance. Although girls frequently do better than boys on tests which demand certain types of memory it is by no means certain that they are able to excel in every type of memory. Bassett reports that girls are slightly *inferior* to boys in retaining historical information; that boys show greater retentiveness for information relative to war, fighting, and geographical location.[16] The girls on the other hand were

[13]G. M. Whipple, *Manual of Mental and Physical Tests*, Pt. ii, 1915, 398.

[14]J. A. McGeoch, The influence of sex and age upon the ability to report, this JOURNAL, 40, 1928, 458-466; The relation between different measures of ability to report, *ibid.*, 40, 1928, 592-595; Intelligence and the ability to report, *ibid.*, 40, 1928, 596-599.

[15]Quoted by Whipple, *op. cit.*, Pt. i, 239.

[16]S. J. Bassett, Sex-differences in history retention, *School and Soc.*, 29, 929, 297-298.

superior in retaining historical facts which treat of domestic conditions and home life. It seems therefore that the type of material has a marked influence upon test results and that the current notion that girls are superior to boys in 'verbal memory' may have been due to the use of terms that are too inclusive.

This tendency to employ loose or inexact terminology probably accounts for many of the other conflicting results that are discussed by Whipple.[17] Whipple states that conflicting results have been found with reference to such abilities as "uncontrolled association," "discrete association," "analogies test," "computation," "logical memory," and "size-weight illusion." The preceding remarks are probably applicable also to such vague and unanalyzed traits as "immediate memory," "delayed memory," "mechanical ability," etc.

(7) *The assumption that group-differences are inherited predispositions.* It was mentioned above that certain differences in the strength of the sexual impulse are probably of sociological rather than of biological origin. This difference and certain other differences have been assumed to be biological in origin. Examples of such assumptions are numerous. Thus, Jastrow has mentioned general paralysis as a typically masculine insanity. He seems here to imply that the greater frequency of general paralysis in men is a clue to the existence of some innate sex-difference in neural functioning.[18] Hollingworth has pointed to the fact that general paralysis occurs more frequently among men because syphilis occurs more frequently among them.[19]

In a forthcoming article the writers present data with reference to sex-differences in play behavior.[20] A list of activities in which girls were found to participate much more frequently than boys was assembled. A second list of activities in which boys were found to participate much more frequently than girls was also prepared. The writers attempted to identify in their two lists those activities which were most likely to demand an appreciable amount of mechanical skill and motor ability. It is of course true that the preceding terms are vague and that almost any activity involves some degree of motor ability. It is apparent however that some activities require much more mechanical dexterity than others. When generous allowance is made for erroneous judgment in the above regard it was evident that the boys participated much more frequently than the girls in activities requiring motor skill or mechanical ability.

Of course various interpretations might be placed upon the writers' findings. It is obvious that differences in background of the sexes are in themselves very marked and perhaps sufficient to account for the differences in types of play behavior. It is conceivable that a list of motor activities might be assembled which would elicit more frequent participation among girls than among boys.

Although it has been found by several investigators that boys and men are superior to women and girls in tests which posit mechanical ability and certain types of motor skill it is very doubtful that the so-called mechanical ability and aptitude tests are valid measures of *innate* abilities.

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[17] *Op. cit.*

[18] J. Jastrow, *Character and Temperament*, 1915.

[19] L. S. Hollingworth, Sex-differences in mental traits, *Psychol. Bull.*, 13, 1916, 382.

[20] H. C. Lehman and P. A. Witty, Sex differences: interest in tasks requiring mechanical ability and motor skill, *J. Educ. Psychol.*, (forthcoming).

To the writers it seems obvious that mechanical ability and motor skill are not unitary traits, but generic terms which include many specific functions or traits. This hypothesis is supported by the findings of Perrin,[21] Muscio,[22] and Barton.[23] These workers found that relatively simple motor abilities correlate only loosely with each other. The question whether the so-called mechanical aptitude tests measure anything more than acquired abilities is therefore a valid one to raise. Do these tests measure *innate ability* or do they test merely specific acquired modes of response? If the tests of mechanical ability are merely measures of previous experience and habit acquisition, one is not justified in designating them motor or mechanical ability tests unless one grants that the ability is an acquisition and not an innate predisposition. It is apparent that girls are more skillful than boys in sewing, knitting, crocheting, etc. This does not mean however that men *cannot* become as competent as women in these tasks with *equal practice and motivation*. Measurement of sex-differences in the above 'abilities' would not be measurement of innate ability but merely a statement of present attainment.

The nature of certain motor activities participated in by boys and by girls suggests that the varied experiences and social stimuli are potent forces in effecting such differences as are brought out by the so-called mechanical aptitude tests. The girls were found to participate very much more commonly than the boys in such activities as "cutting paper things with scissors," "stringing beads," "sewing, knitting, crocheting, etc." The boys on the other hand participated much more frequently than the girls in activities such as "using a hammer, saw, nails," "making or using a wireless or other electrical apparatus," "throwing rocks or stones," "climbing," etc. These differences in recreational interests may be traceable to social customs, attitudes, and values. So conspicuous are these factors that they suggest that alleged innate sex-differences in mechanical ability may be due to differences in attitude and experience, products almost solely of environmental factors.[24] It may be that sex-differences in performance in the mechanical aptitude tests are therefore not tests of *ability* at all but merely tests of the present level of performance. Perhaps certain fundamental assumptions of those who presume to test these 'abilities' will need to be revised or modified.

Results of some major investigations. There was a period not far removed when "male superiority" was proclaimed and generally accepted. More recently it was believed by many that important sex-differences were to be explained by the fact that the male is by nature more variable than the female. For example, as recently as 1926 Havelock Ellis defended his earlier position

[21] F. A. C. Perrin, An experimental study of motor ability, *J. Exper. Psychol.*, 4, 1921, 24-56.

[22] B. Muscio, Motor capacity with special reference to vocational guidance, *Brit. J. Psychol.*, 13, 1922, 157-184.

[23] E. Barton, *Correlations among Motor Abilities*, A. B. Thesis, Univ. of Wisconsin. (Quoted by C. L. Hull, *Aptitude Testing*, 1928, 21.)

[24] It is of course obvious that, in so far as *general* mechanical ability is concerned, sex-differences may be (and probably are) due merely to the weighting of the test in favor of one sex or the other. The earlier reports with regard to sex-differences in general intelligence are suggestive in this connection, and the fallacy of these earlier reports should not be ignored.

concerning the relative variability of men and women.[25] Experimental evidence reveals that whenever a large population of a given age has been tested, girls are as variable mentally as are boys. For example, in revising the Binet–Simon Test, Goddard studied 2000 children. His data failed to show any sex-difference in variability.[26] In administering his series of arithmetic tests to large numbers of New York City children, S. A. Courtis likewise found no sex-difference in variability.[27] In revising the Binet-Simon Scale, Terman studied 1000 unselected children; the supposed greater male variability was not found.[28] In standardizing his Language Scales, Trabue studied 1590 boys and 883 girls. His figures show that (according to quartiles) there is no sex-difference in variability.[29] Frasier studied the grade location of all of the thirteen-year-old children in 20 American cities. The greater male variability was not demonstrated in his analysis of 62,219 boys and girls. The number of boys and girls at the extremes of the distribution were approximately the same. Moreover, the range was the same for the sexes.[30]

General intelligence and educational tests have thus far not been standardized separately for the sexes. From available data, it appears that sex-differences do not warrant such a practice.

In so far as school needs are concerned, E. A. Lincoln points out that increased provision for individual differences meets adequately the problems of sex-differences.[31] To the psychologist the problem of sex-differences nevertheless continues to be a pertinent one. In view of this topic's unfortunate history and in view of the numerous possibilities of spurious findings and erroneous conclusions, reported sex-differences should indeed be scrutinized critically.

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FIRST INTERNATIONAL CONGRESS ON MENTAL HYGIENE

The Congress is announced for Washington, D. C., May 5-10, 1930, under the presidency of Dr. William A. White. Annual meetings of the American Psychiatric Association and the American Association for the Study of the Feebleminded will be held concurrently. The subjects for discussion in the Congress bear upon mental hygiene at all ages of man, upon the family and the community, upon instruction, and upon care, prevention, and treatment. Various related subjects, anthropology, education, eugenics, psychology, nursing, sociology, etc., are also to be represented upon the extensive program of the Congress.

M. B.

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[25]H. Ellis, *Man and Woman: A Study of Human Secondary Sexual Characteristics*, 6th ed., 1926. See new preface.

[26]H. H. Goddard, Two thousand normal children measured by the Binet Measuring Scale of Intelligence, this JOURNAL, 18, 1911, 232-259.

[27]S. A. Courtis, Report on the Courtis Tests in Arithmetic, *N. Y. Committee on School Inquiry*, 1, 1911, 391-546.

[28]L. M. Terman, *The Measurement of Intelligence*, 1916.

[29]M. R. Trabue, *Completion Test Language Scales*, 1916.

[30]G. W. Frasier, A comparative study of the variability of boys and girls, *J. Appl. Psychol.*, 3, 1919, 151-155.

[31]E. A. Lincoln, *Sex Differences in the Growth of American School Children*, 1927.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Systematic Psychology: Prolegomena. By EDWARD BRADFORD TITCHENER. New York, Macmillan, 1929. Pp. xi, 278.

Psychologists who are familiar with Titchener's long and productive term of academic service will remember that his chief university courses were two; one an Introduction which stands represented by his text books and laboratory manuals, the other a Systematic Psychology, extending first through a year's lectures and later through two years. Early in his teaching career the Systematic lectures were written substantially under the outline of Külpe's *Grundriss* (1893). The general constitution and program of these lectures were not much changed through thirty years and more; although considerable new material, factual and theoretical, was added during this long period. Titchener himself relinquished the course about 1900; but for a great many years he cherished the hope that he would at some time bring to publication in a large work what he called his "system." Various phases and by-products of this system appeared in occasional lectures upon Feeling and Attention and upon the Thought Processes, in many articles and in the textbooks, the latter always cast in strict systematic terms. The great work, however, for which the model always stood before him in Wundt's *Grundzüge*, never appeared. Outlines, segments and parerga in plenty; and now we receive with high interest the *prolegomena*.

The volume opens with a comparison of Wundt and Brentano, of empirical and experimental psychology. A chapter devoted to the general nature of science and two upon the definition of psychology (point of view and subject-matter) make up the body of the book. The lesser half of the work (pp. 3-22, 157-259) is reprinted from articles published in this JOURNAL in 1921-22 (Vol. 32, 108-120, 519-542; Vol. 33, 43-83). It includes the comparison of Brentano and Wundt and most of the chapter on subject-matter. The new sections chiefly treat of point of view and of science taken in the large. A few pages added to the last chapter include in general summary the author's views respecting the proper relations of psychology to physics and biology.

The republished sections of the book call for no review and for comment only where they are integral to the entire work. They are in the main critical expositions and estimations of other systems, most of them from the 19th century. Those writings which fall in part in the earlier years of the present century but derive largely from earlier times include the names of Münsterberg, Meinong, Stumpf, Husserl, Lipps, Külpe, Witasek, and Höfler. Angell was roughly contemporaneous with Titchener's 'structural' period and Messer follows in the traditions of Husserl and the later Külpe. All of this systematization which Titchener expounds and values was either done or else in the baking before 1910. It is perhaps natural, then, that the psychological ideas among which Titchener moves in his discussions belong—nearly all of them—to the third-of-a-century following 1874 (Wundt's *Grundzüge* and Brentano's

Psychologie vom empirischen Standpunkte). A biological hint or two comes from a later time; but there is scarcely a suggestion of the late revolution in physics or of recent matters in psychology.

The author skilfully expounds Brentano and Wundt, himself voting heartily for the experimental Wundt and the Wundtians, who *describe*, and against Brentano and his empirical kind, who *argue*. Had he considered fruits more than formal conceptions he might have recalled respectable descriptions of an experimental sort which came from Brentano's great disciple Stumpf, who (together with his laboratory students) filled for many years one of the meatiest sections of Titchener's own lectures upon Systematic Psychology.

In the chapters which follow the Introduction Titchener proposes to define psychology, and since he desires to exhibit his own subject as a science among others he proceeds to derive a conception of science-at-large which shall bring all the fundamental sciences (including psychology) to a common ground. This common ground he finds, as he thinks, both in point of view and in subject-matter, terms in which he had ordered his scientific thinking for at least thirty-five years.

Titchener's occupation with 'system' was almost an obsession. All systems —his own and others—he constantly tested in terms of point of view, subject-matter, and method. 'Method' (of which he wrote much and with great conviction in his more productive years) he did not do for the present context. For him it always logically followed subject-matter. The real reason why he did not again, in the last ten years, 'tackle method systematically,' as he might have said, is not easy to assign. The task certainly was less difficult in those years when he was devoted to 'introspection' and wholly confident of the general competence of that method.

In reading the chapter devoted to science, those who have known the author will find presented much that they already knew of Titchener's general views. The man of science is as important as the material, the point of view and the method. "We choose as our starting point," he writes, "the scientific temper or attitude or frame of mind" (28). This is (on the negative side) *dissent*: "the man of science, then, is out of accord with the majority of his fellow men" (30); and (on the positive side) *observance*, a "disinterested and impersonal observance of the world of human experience." The man of science is "the servant of nature: interpretation he leaves to those of different bent" (34). Interpretation includes such things as valuation, meaning, and human uses. Bare existence only is left. "An instinctive determination to identify himself with his subject-matter, to lose himself in it, to become one with it." Does this echo an early revolt in student days and the espousal of the righteous and protestant cause of science when science and religion made warfare? The English references cited in the context seem to suggest it. At all events the implication is important; for the contention reveals our author's formal attitude not only toward psychologists and systems but also toward many other men and matters. It is a kind of priestly dedication. Its *credo* seems to imply (quite against the logic of the man of science) a doctrine of human, intellectual and professional values. It is possible that men of Titchener's social orientation are easily inclined toward such a position.

When we turn from the man to the materials of science we learn that the fundamental matter is "an intensive living-through of some item of the existential universe. . . . . a sort of participation" (39). At the same time "the attitude of science is . . . . . complicated by the non-scientific, extra-scientific attitude of logic" (44). The large part necessarily played by logic is obviously disturbing to Titchener, who insists that the special competency of the man of science is "his competency as observer" (49). The disturbance may have been accentuated by his reflection that rigorous observation at first-hand and in an experimental setting had occupied but a very small place in Titchener's own scientific labors since the Leipzig days and what he calls 'logic' a very large and preponderant place. "Observation is," as he remarks, "the single and proprietary method of science and . . . . . experiment, regarded as scientific method, is nothing else than observation safeguarded and assisted" (43). Whether this single and proprietary method can safely be delegated to others by the man of science is not discussed in the book; although the author does suggest that "we might . . . . . ask the logician to plan our experimental procedures and the man of science to make the relevant observations" (44). Had Titchener not so wholly committed himself to 'existence' and had he not so thoroughly "interwoven . . . . . observance, existence and observation" (45), his expository way would have been easier and more 'logical' and his account of science more internally consistent. He will not desert the ancient dichotomy of 'fact' and 'logic' and nevertheless logic will not stay out of, nor can it come in to, his science.

Because of its important consequences we must pause upon the corollary that all description starts with analysis. "It is universally agreed, then, that the first problem of science is analysis" (58). Whence the "then?" The only justification which the reviewer finds in the text is that direct acquaintance-with "can be gained only piecemeal, little by little." But when has this contention been "universally agreed" upon? Many men proceed otherwise. Indeed the prescription seems to rest rather upon some such logical assumption behind observation as a doctrine of physical or of mental 'elements' or as a genetic theory of the derivation of the complex from the simple than upon necessity, either logical or scientific. It falls in nicely with the older methods of teaching chemistry and, in psychology, with a first descriptive chapter devoted to sensations; but it stands squarely against the author's own conception of biology, where the unit is the organic individual, not to be known characteristically by an analytical split-up but by the observation of its total dependence upon 'environment' (see below). In Titchener's own complacent and individualistic psychology analysis was a cardinal principle of procedure, a principle to be enforced by declaration and by precept; but when it is arbitrarily prescribed for all science that is 'really scientific' it wears the appearance of a dogma.

The fundamental sciences he assumes rather than shows (87) to be three, physics, biology and psychology. All are at bottom observational. While they must combine 'logic' with observation, logic is always secondary and the observational acquisition of facts primary. Their common ground is 'existential experience.' In psychology this experience is logically dependent upon the nervous system, in biology it is logically dependent upon a physical environment, and in physics it is logically interdependent but externally independent. The three subject-matters are diverse but related. On the 'material' side,

physics is energetic, biology behavioral, and psychology sensory; on the 'formal' side, physics is non-individuate (or universal), biology is individuate, and psychology is systemic, *i.e.* it correlates with a single organic system (264). When stated in terms commoner in the history of the sciences, the objects of physics comprise an entire 'order of nature' with interdependent masses and energies, the objects of biology are limited to individual living beings which sustain relations of dependence upon non-living objects and agencies, and the objects of psychology are sensory phenomena which appear only in conjunction with neurological (*i.e.* biological) objects and processes. In still grosser terms we may say that the materials of physics are all-of-a-piece and interactive, those of biology are, so to say, variegated clots of existence which presuppose physical objects and agencies, while those of psychology are sensory facts functionally related to the body. Titchener is extremely non-committal on the "material characterization of psychological subject-matter," only venturing "sensory" as a "leap in the half-dark" (265). It is extraordinary that when he comes to the momentous question 'What after all is the stuff of which psychology is made?' he virtually gives up with a thin page and a 'postponement' which must be "altogether indefinite" because "the very nature of these facts is in dispute" (265).

Titchener's conception of and his practice in psychology are thus substantiated by a generalization looking toward biology and physics. Here British science, from Spencer and Darwin down to Karl Pearson, is chiefly called to witness. If psychology demands an observational description of the existential sort, so do physics and biology. Thus runs the argument. But the argument shifts in the last two chapters, where definition is the main theme. Here physics tends to take the lead and biology becomes a major problem. Psychology then is given dignity and rank by its ability to fit into a place coördinate with these other subjects.

One of the main objects of the book seems to be to bring biology into the general logic of the sciences; and the author thinks that he has solved this problem in a new and satisfactory way. To relate physics and psychology, proposals in plenty (such as they are) have been made; physical and spiritual, extended and non-extended, conscious and non-conscious, mediate and immediate, and many more. He reviews them all and finds none satisfactory and none that leaves a coördinate third place to biology. So he proceeds "to write a differential formula for psychology in terms of its relation to physics and to biology" (88). Physics itself gives superficial and naïve solutions. Philosophy and psychology have taken the task more seriously. There are proposals by Wundt, Avenarius, Mach, Ward, Külpe, Ebbinghaus, and James. That of Avenarius is most engaging. His proposal to throw over a special subject-matter (consciousness) and to define by point of view is, as Titchener writes, "satisfactory; and yet we cannot say that we are satisfied" (137). Avenarius's 'experience' included value. Here Wundt is "a far safer guide" as he leads toward an existential experience. If we then regard this sort of experience as dependent upon a determinate biological system (nervous system) we shall attain a 'satisfying' point of view for psychology. Furthermore, biology's point of view is of the same sort; existential experience regarded as dependent upon a determinate *physical* system, *i.e.* environment. This last dependence Titchener looks upon

as a discovery "not hitherto . . . . . proposed" (139). Yet he cites Charles Darwin and L. J. Henderson and he talks of 'evolutionary process,' a functional 'complement' to the organism, 'maximal freedom,' and 'suitability.' All operational, functional, adaptive terms! The biological concept of 'environment' would seem to be the last that Titchener would have consented to join with an 'existential' point of view. He must have been misled by Henderson's physico-chemical terms and he must have overlooked Henderson's 'suitability.' Again, it may be doubted whether this is really "definition of biology by point of view" (141). After all it is the *organism* which is environed and without the organism there is (as Titchener agrees) no environment. The organic subject-matter would seem then to determine biology and the point of view to have been secondarily derived from the operational notion of a sustaining and 'promoting' environment. This is by no means (nor has it been since the days of Lamarck and St. Hilaire) the *logically* dependent member of an existential relation.

But if we could grant that this is really a logical dependence and is a true determination by point of view, we might still ask whether the science of which Titchener writes is 'biology.' In some passages, biology is obviously the 'general biology' of evolutionary fame, in others Titchener seems to be providing in general for the sciences of life, and again he has before him the living individual in its inorganic setting (biological existence is "individuate and behavioral" (263-264). Titchener cannot abide 'behavior,' which is valuational, and 'life,' which is institutional and practical. His own terms are softer and the reader may not at once discover their real import. Had the author dealt frankly with his sustaining environment he would have taken a hint from his behavioristic enemies and provided for the opposing term of the interaction, thus bringing in *response-to* as candidly as he had *stimulation-from* the environment. His demand for the existential everywhere blinds him to the double intercourse of living things with 'physical nature.' When we recall his own stand for 'sensationalism,' with its British affiliations, we can readily apprehend his liking for the 'impressional' side of matters, both in psychology and in biology. In fact his conception of existence seems to imply a description of the passive and the receptive. To be sure, he makes physics energetic; but then he does not work out his physics and especially show how a subject-matter energetic in form might be existentially described. And had he seen that the environmental relation is only a special case of *text* and *context*, of dealing with the materials of science as variously related, he might have provided for physiology, anatomy and embryology as well as for ecological aspects alone of living beings. Internal contexts, related organs and systems, and developmental stages are quite as important (whether considered in 'biological' or in 'physico-chemical' terms) as the outside dependence, which Titchener vainly tried to make purely 'logical.' In one place (262) environment is made to include "the organism itself, in so far as organic phenomena are not individuate but energetic." This troubles Titchener's logic, for a thing can scarcely be 'dependent' upon itself. He provides here neither for those biologists who *biologize* internally nor for those others who regard their whole business as plain physics and chemistry.

His obsession with British biology of the Darwinian type led him to stress a purely local and practical line of demarcation at the rind and limit of the body to the neglect of a general distinction of text and context which would have

carried straight through his three sciences. The context for psychological facts would have been bodily and for biological facts both bodily and inorganic, while physics would either have supplied its own context, or been supplied from biological and psychological sources, or have remained (when taken as an entire system) only text. And without his heavy concern for 'biology' the arbitrarily assumed trinity of the sciences might have been dropped and such subjects as physiology, geology and galactic astronomy accorded a judicial hearing.

Those readers who know the author's intellectual history will probably agree that the main interest served in the chapters carefully brought together by Professor Weld is the contention that science is the existential description of experience. The contention involves an observer who is in dissent from, and in revolt against, many cherished views and interests of man. The observer eschews the practical, the technological and the valuational, and he identifies himself with the immediately observed. The observed is always experience and experience regarded as 'functionally or logically' dependent or interdependent. We know what all this meant in terms of Titchener's psychology. It never covered the entire field save by the author's arbitrary delimitation. Were he to apply it to his science of the organism, it would seem to mean a kind of passive ecology. What it might produce in the physical sciences is very difficult to say. In fact, it is not at all certain that the common basal term *experience* carries the same meaning in the author's three sciences. He nowhere shows that it does. Although most men who are devoted to the sciences would like to think of their work as a serious, straightforward and objective business, I do not believe that we should find in most of the works of science very much which conforms in strictness to Titchener's formal conceptions. And it is a striking fact that our author was himself a collector, a classifier and a critical arranger of facts, men, objects and systems—all matters (as he says) of logic. Even in his own experimental problems he delegated most of the observing. This very proficiency in sorting, distinguishing, bringing under rubrics and setting into order seems to the present reviewer to have determined the main virtues and uses of the book. The book is full of information well arranged. The psychological reader who fails to understand it, to delight in its diction, to be vastly informed by its historical lore, and to be quickened by its logic in his own scientific and professional thinking cannot reasonably regard himself as a master in his subject. Whether the *Prolegomena* would have led on to a rounded and useful 'system' can scarcely be predicted. Since they fairly present the author's general background for his earlier decades, it may be well to let the present essays stand as a reasonable and enlightening *apologia* to the *Outline*, the *Textbook*, the *Experimental Psychology*, and to much beside in the author's large contributions to his subject.

Cornell University MADISON BENTLEY

The Psychological Register. Edited by CARL MURCHISON with the cooperation of an international board. Worcester, Mass., Clark University Press, 1929. Pp. ix, 580.

The *Psychological Register* is a Who's Who in Psychology. In it are listed, with a few notable exceptions, the names of all the psychologists in the world

of sufficient standing to merit recognition by their confrères. Besides biographical items—full name, address (usually of 1927), place and date of birth, academic and professional history—the *Register* gives a more or less complete bibliography of every individual.

The names are arranged alphabetically by 'country' and alphabetically within every one of those divisions. The classification by 'country' does not, however, follow any fixed principle. It is based upon the political state in which the individuals reside, as for example, Austria, Belgium, China, Czechoslovakia, Denmark, Finland, etc.; but this principle frequently gives way to linguistic and geographical considerations. The free State of Danzig is included with Germany, Portugal with Spain; Canada is grouped, not with the other Dominions under 'British Empire,' but with the United States under 'America;' Argentina and Brazil are grouped together under 'South America,' though they are the only two states represented from that continent. The psychologists of the British Empire, with the exception of the Canadians as noted above, are grouped together in a single alphabetical classification. It is, consequently, difficult to obtain from the *Register*, without much labor, a list of the psychologists residing in Australia, England, India, New Zealand, Scotland, South Africa, and Wales. The *Register* would have been more serviceable had a single principle of classification been employed—preferably on inclusive alphabetical arrangement followed by a geographical index.

The volume closes with an index of names; consequently the reader may find the relevant facts for any given individual though he may not know beforehand the country in which the individual lives or is listed. The expedient thing to do when looking for an individual is to turn immediately to the index. Among the names that the reviewer misses are I. P. Pavlov of Russia, Sigmund Freud of Austria, C. G. Jung of Switzerland, E. Rubin of Denmark, F. M. Urban (formerly of the University of Pennsylvania) of Czechoslovakia, R. H. Wilcocks of South Africa, G. Humphrey of Canada, J. E. Evans, G. M. Fernald, D. A. Laird, D. B. Leary, and A. H. Sullivan of the United States of America.

The *Register* is printed in small type (8 pt. solid), and in double columns. The authors' names are printed in boldface, and stand out distinctly from the body of the page. The surnames are, as a rule, printed in capitals; the given names, in capitals and lower case. Many exceptions were, however, inadvertently made. The prefixes, De (pp. 56 f.), Le (p. 385), Mac (p. 144), Mc (pp. 149 f., 328), etc., are printed in upper and lower case. This is inconsistent with good usage; small capitals instead of lower case letters should have been used. Again, surnames with accented letters are printed in large and small capitals, the accented letters being printed small (cf., for example, pp. 370, 486, 539). The effect is unpleasant and good usage is further violated. The editors should have insisted upon the printers using type of one size.

The biographical facts given after the individual's name are sufficient for ordinary needs, the salient items reported showing the individual's training and the extent of his influence. Information concerning marriage and children might profitably have been given. The biographical information is printed in English, French, German, Italian, or Spanish; other languages are usually replaced, though not always, by English. In the reviewer's opinion it would

have been better had English been used throughout; the book is published in English and it will be chiefly used by English-speaking people.

The bibliographies (translated into English in the case of Russian, Japanese, and Chinese, and given in the original in the case of other languages including the Scandinavian) are uneven and incomplete. Most of them were furnished by the authors, but many, as the Preface relates, had to be prepared in the editorial office. The latter are naturally less complete than those prepared by the authors, but they suffer from a further limitation in that they do not extend back of 1894, the date of the first *Psychological Index*. The bibliographies prepared by the authors differ also in completeness. Some of the authors, in accordance with the editors' request to list only important studies, prepared a selected bibliography; others, ignoring it, included everything they had written. The request for a selection was unwise, not only because it made for inequalities, but also because it eliminated from the bibliographies of those who complied with the request the very items that usually escape indexing, and that are, as a consequence, difficult to discover.

The limiting dates of the bibliographies vary from 1927 to 1929. Here, as elsewhere in life, the men who were on time suffered; bibliographies which were returned when called for were cut short at 1927. The dilatory, who returned their bibliographies late or who had their bibliographies prepared in the editorial office, profited; their bibliographies were brought down to 1928 or 1929. Good editorial practice, to say nothing of justice, demands that the limiting date should be the same for all.

The references given in the bibliographies are frequently incomplete. Sometimes the date, sometimes the volume number, and many times the page references are lacking. Where page references are given, various styles of citation are followed: inclusive pages, beginning page and dash, or beginning page alone.

The American section, which runs to 296 pages and forms over half of the book, is the best edited of all the sections. It is not, however, free from error. All the errors mentioned above find ready illustration in it. The most frequent error, however, is the omission of pages from book references. The poorest edited sections of the book are the Austrian, in which the various styles of citation follow one another in haphazard order (cf. p. 303), and the Belgian and French, in which volume number and page are omitted from most of the citations. The differences in editorial style should be smoothed out before the appearance of a second edition. Date, volume, and inclusive pages should be given in every reference.

The proofing was on the whole carefully done. The errors noted above are all vagaries in editorial style; they cannot be charged to the proofreaders. The following errors must, however, be charged to them. Wrong cases are used in printing the authors's names on pp. 51, 299, and 302. Accents are omitted in Hoernlé (p. 324) and Juhász (p. 489). Accent and 'von' are omitted in Stephen von Máday (p. 490). The umlaut is omitted in Charlotte Bühler (p. 297). J. P. Nafe's first article (p. 168) is on the 'affective,' not the 'effective' qualities. P. C. Squires (p. 222) is incorrectly placed as at the "Univesity of Kansas in Los Angeles, Calif." This shift of the University from Lawrence, Kansas, may be a bit puzzling to non-American readers. E. W. Scripture

(p. 210) and G. H. Wang (p. 262) are incorrectly included in the American section. As their bibliographical data clearly indicate, they belong respectively to Austria and China. A few names, moreover, are incorrectly alphabetized (pp. 161, 196, 361, 377, and 402).

The *Register* nevertheless is a valuable book of reference. It cannot of course be used to the exclusion of other indexes; but it supplements them. It gives information that can be found in no other place. The psychologists of the world owe the editors a debt of gratitude for undertaking its publication.

Cornell University KARL M. DALLENBACH

An Historical Introduction to Modern Psychology. By GARDNER MURPHY, with an appendix by HEINRICH KLÜVER. New York, Harcourt Brace & Co., 1929. Pp. xvii, 470.

There has never been a thorough-going history of modern psychology, of psychology as it exists to-day—mostly in laboratories—in its historical setting. Brett wrote of it in his third volume, but not from within it. Klemm constantly touched it. Pillsbury has very recently included much of it. At first glance it would seem as if Murphy had supplied the need. However, he calls the book *An Historical Introduction to Modern Psychology*, and not the History of Modern Psychology. Is the title correct?

Essentially it is, but the book is also, in a way, a history that debouches from the past toward the present, so that when it is done all of the very most recent psychology has been arrayed along the entire modern front. Most histories begin to become dim within the point of near-vision; historical perspective, however, is not for Murphy an end in itself. Murphy wants to write about the present, and to put it, which has no perspective to itself, into a kind of perspective by showing it as the latest stage of a genetic development.

The first one-sixth of the book is all history and atmosphere. Murphy gives a bit of the intellectual background of the seventeenth century, devotes a chapter to the seventeenth and eighteenth centuries, writes another chapter of the nineteenth century on Herbart, phrenology, and the French and Scottish schools, and concludes with a discussion of the general intellectual antecedents of experimental psychology in the other sciences, in philosophy and in wider intellectual movements. If this part and the next of the book have been flavored by Merz's *History of European Thought in the Nineteenth Century*, that fact is no disgrace to the author.

The second section of the book, about a quarter of the whole, is called "From Weber's Experiments to the Age of Wundt." It deals with Weber and Johannes Müller, with Hamilton, the Mills, Bain and Spencer, with evolution from Erasmus Darwin through Charles Darwin and Galton, with psychiatry from Pinel and Mesmer to Charcot, and then with the experimental psychology of Helmholtz, Lotze, Wundt, Cattell, and a few others. Cattell is misplaced, and G. E. Müller and Stumpf are neglected, but in general all this part is history too.

The third and last part occupies a little over half of the book. While Murphy does not seem to me always to have been fortunate in his chronological divisions, this part can be said to consist of sixteen chapters each of which presents some current phase of psychology in its historical development from about 1880 or

some later date. The topics are: memory, James, structural and functional psychology, thought, learning, behaviorism, child psychology, social psychology, the psychology of religion, psychoanalysis, instinct, intelligence, personality, physiological psychology, and contemporary German psychology. The two chapters on the last topic form an appendix written by Klüver. It is apparent at once from this list that the book does not progress as a whole. One has in the second half of the book a collection of historical accounts for different topics. Perhaps this effect is inevitable, for history has the two dimensions of breadth and time, and language has only the one dimension of progression. In writing history an author cannot go both ways at once, and Murphy has not neglected to pick up connecting threads of woof as he advances along one line, although he can hardly be said to have woven the strands into a single fabric.

I am being critical, for such is the function of the reviewer. I think Murphy's book is the best history of psychology that has ever been published for the use of modern psychologists, and the reader must remember that he does not claim that it is actually a history at all. But, if perfection be the standard, he seems to me just to have failed of integration. This failure is due in part to the chronological 'zig-zag' which characterizes this latter half of the book.

The 'zig-zag' is also the method of progress within the chapter. One could take almost any chapter, but let us examine the one on Child Psychology (pp. 279-288). After a brief introduction one is started in with Preyer (1881) and in two pages has advanced to Thorndike (1914), then one is brought back to Stanley Hall (1904) and to Groos (1896) and forward to Piaget (the present), back to Witmer (1896) and forward to Watson (1917). Would not the student of the history of child psychology have to get out paper and pencil and rearrange all these men in chronological order to understand what had really been happening? Murphy writes principally about the last fifty years, and he writes of them, except for his explicit tracing of genetic relations, as if this half century were all one big conscious present.

We should next consider the scope of the book and hence the brevity of the treatment of individual topics. Murphy's scope is as broad as it well could be, if the more philosophical aspects of psychology are left out of account. One senses his interests. He moves away, where he can, from physiological and experimental psychology toward the social psychologies, from systematic psychology toward the applied psychologies, from the nervous system, sensation, perception, feeling and emotion toward psychoanalysis, instinct, intelligence, and motivation. He has the right to choose; no author could be omniscient. However, one feels that he prefers the broader reaches of modern psychology to the closer atmosphere of the laboratory.

On the other hand, he has by no means neglected the laboratory, and the resultant range of the book makes his treatment of most topics very brief. When I began the task of this review, I tried, with the malign instinct common to reviewers, to find the names of important men who were entirely omitted, and succeeded very badly. Nearly all the great are named. Then for a while I was admiring Murphy's condensation, but after a time this brevity began to pall. Hardly ever could I get any real information about any topic. William James has a chapter and is well done, and there are some other like places; but in general one moves on to the end, picking up crumbs with never a bellyful.

Well, what was Murphy to do? He could not write fully of so broad a field in four hundred pages. He made his choice, and I am merely recording my impression of the result without saying how the fault should be remedied.

I think Murphy is accurate, or else I must disclaim expertness in some historical fields where I thought I was well versed. In the entire book I have noted nothing that I should call a certain error of fact, except his placing (pp. 162 f.) of Wundt's tri-dimensional theory of feeling in 1893 when the date should be 1896 or 1902. This date is unimportant as Murphy mentions it; it could be important in fixing the effect of Wundt's theory upon contemporary research.

Murphy's literary style is unusually good as compared with most psychological writings in the journals, and in books too, of the present day. Of course, he has not the felicity of William James; none of us has. But he never calls attention to himself on account of his phrasing, and he has succeeded in never once offending the ear of a critical reviewer. He may have worked hard for perfection in an age when few psychologists do, and he ought to be congratulated.

Not so much can be said for Klüver's appendix. Klüver's treatment of contemporary German psychology has the ring of an expert, as those who have read his articles well know. However, the sentences are too long and the paragraphs much too long. There are at least two paragraphs (pp. 429 ff., 448 ff.) that are almost three pages in length. Of course Klüver is not writing in his mother-tongue, but that fact does not prevent his reader from feeling that the reading is more of a duty than a pleasure. Murphy leaves one comfortable if hungry: Klüver fills his reader but makes him wade for his dinner.

In another way, too, Murphy obliterates himself from the book like a perfect host, but it is impossible to read Klüver and to forget the author. For instance, Klüver gives long lists of names separated by commas—fifteen names in one place (p. 431) and eleven in another (p. 438). These names have no use. If the reader knows what the men in the given context stand for, he knows too much to read the chapter. If he does not know, he is certainly not being told, especially as there are no foot-notes with references, or even initials, to start him on an investigation for himself. The result is that the reader feels that the author failed to suppress a show of knowledge where its exhibition had no meaning, and he concludes that the author is not really his friend.

The book is not the best sort of reference work. There must be about nine hundred foot-notes in it and nearly all of these are references. These citations seem to be accurate as far as they go (though Klüver cites a misleading edition of Mach, p. 430), but they lack initials for the author cited and all page references. One gets surname, title and date only. Everyone knows how provoking it may be to be referred generally to a book and to be left to find his own page. Moreover, although there may be nine hundred references, there probably should be two thousand. One is left again and again without the desired documentation, either for a man's name or for a particular topic. Murphy has done well in this matter, but he could have done better.

One other matter that makes me uncomfortable in reading this book is Murphy's habit of translating the titles of books. The main discussion of Lotze does not even mention his famous book, but in two other places it is cited as his *Medical Psychology*. The *Medicinische Psychologie* was never even

translated, and, if Murphy is going to translate the words, why should he not translate the meaning too? Lotze did not write in 1852 a medical psychology as those words connote to-day; he wrote, in the phrase of his own subtitle, a *Physiology of Mind*. There are lots of other instances where the English title sounds odd (*e.g.* Descartes, p. 8; Leibnitz, p. 11, Herbart, p. 46, Preyer, p. 280).

All this is general. I can now turn to certain criticisms that forced themselves upon me as I read. The reader must, however, not misunderstand me. I can nowhere say flatly that Murphy is wrong. The very nature of my criticisms is a compliment to Murphy.

Fechner (p. 52), I think, got the notion of the limen from Herbart and the notion is not so different from Herbart's. Titchener thought so. J. Bernstein was influenced by Fechner, possibly through A. W. Volkmann, and his diagrams of the limen of consciousness closely resemble Herbart's.

It is quite proper to give Charles Bell his due for the law of the spinal nerve roots (p. 78), but it does not seem to me quite fair to reduce Magendie to fourteen words in a foot-note when the Bell-Magendie law is under discussion. Hardly anyone thinks that Magendie cribbed, even unconsciously, from Bell, and anyhow the introduction of the law into France is important, for France was then the important nation for scientific physiology.

Certainly Weber originated Weber's law (pp. 79-85), but a reading of *Der Tastsinn und das Gemeingefühl*, or of Lotze's and Bain's early comments upon the research, does not convince me that Weber attached the importance to the law that Murphy seems to believe. It really took Fechner to give the law its full significance.

Johannes Müller discovered the reflex in its modern form (p. 86), but he himself admitted Marshall Hall's priority and Murphy might mention Hall.

Carmichael would probably say that Murphy gives too much credit to Johannes Müller for the law of specific nerve energies and too little to Charles Bell (pp. 96-98), but I am not sure that I disagree with Murphy's evaluation. Bell in his obscure little brochure made less of this law than Weber did of Weber's law, and Murphy does mention Bell (pp. 96, 78).

John Elliotson has always seemed to me to be much abused, and I should like to see Murphy give more credit to him, and also to Braid, in his discussion of hypnosis (pp. 141-143). Furthermore, he connects Braid too much with phrenology, and he is hardly fair in saying that it was some time before Braid came to a clear opinion in the matter of 'neurypnology' (p. 142). Braid had reversed his opinion, learned how to hypnotize, and had formed the basis for the view that is attached to his name, within a few weeks after he first denounced the mesmerist, Lafontaine.

Murphy's only harsh phrase in the whole book is his statement that Hering made it his duty to disagree with some authority on everything (p. 153). German polemics do not appeal to the modern eclectic, but Hering does not seem to me to have got his due. It is as if the foot-note about the Ladd-Franklin theory had affected the whole discussion.

Wundt is a difficult topic for anyone to tackle (pp. 160-173), and Murphy seems to me to have done fairly well. There is the error about the date of the new theory of feeling (mentioned above), the repetition of Stanley Hall's

misstatement that Wundt was assistant *to* Helmholtz (p. 160), whereas he was assistant in physiology to give instruction to medical students independently of Helmholtz when Helmholtz was professor of physiology, and a wrong subtraction of dates which places Wundt in Leipzig five years (p. 162) before the founding of the *Institut*.

The introduction of a full discussion of Cattell's work (pp. 173-178) into the 'age of Wundt,' when his contemporaries are left for the modern period, seems to me to be in false perspective. The designation of Cattell as "the most original and productive member of the Wundtian group" (p. 173) is a courting of controversy. Really now, was Cattell more productive and original than Külpe, Meumann, Titchener, Kraepelin, or Münsterberg? I have always thought of Cattell and Münsterberg as persons who were *in* the Wundtian group but not *of* it. Perhaps this divorcement is what Murphy means by 'originality;' but then why does Cattell go into the chapter on Wundt when the others are left out and in some cases distributed elsewhere? Part of the trouble here is that Murphy's chronological 'zig-zagging' leaves the dates of the 'age of Wundt' only vaguely defined.

Murphy relates Ebbinghaus to Herbart but not, in any intimate way, to Fechner (p. 190). He should look up Ebbinghaus' dedication of the *Grundzüge* to Fechner: "Erkenn' ich wohl, ich hab' es nur von Euch." Ebbinghaus, without personal contact with any psychologists, discovered Fechner's *Elemente* in a Paris book-stall, was greatly impressed, probably took the book and the ideas to England where he came into closer contact with associationism, then, in England or France, began his experiments in an attempt to measure memory as Fechner had measured sensation. Of course, the expository structure of *Über das Gedächtnis* makes it look as if Ebbinghaus had gone to physics, and not to psychophysics, for method, but the exposition of that famous book can hardly be the history of Ebbinghaus' thinking.

Brentano is skimped (pp. 233 f.); he deserves more space. He not only set the stage for Austrian psychology, but he determined Stumpf. But then Stumpf is skimped too (p. 181), as is also G. E. Müller (four scattered paragraphs, pp. 168, 181, 197 f., 256 f.).

An entire chapter devoted to instinct (pp. 336-346) is a little shocking, when there is no chapter on visual sensation, or on perception, or on feeling. Is not attention as important historically as instinct?

Psychophysiology is in, but it is scattered. The picture of the nervous system that centers around the neurone theory comes in the chapter on Memory (pp. 199-204), where no one would ever look for it. Related material enters in other places, and hence much of the proper content for the chapter on Physiological Psychology (pp. 395-405) is not where it should be in this series of historical essays.

Murphy ends by summing up the entire history of modern psychology. It shows, he thinks, a tendency away from structures toward functions, away from elements toward *Gestalten*, away from the qualitative toward the quantitative, and in addition the development of the genetic and statistical methods. I cannot quarrel with him in this summary. I should, however, quarrel with his picking out, in this resumé, the normal law of distribution as one of the greatest laws of psychology (p. 413). Murphy still has the majority of psy-

chologists with him, but my guess is that psychology is being retarded by their error. For a similar reason he makes more of the method of correlation (p. 412) than I should dare to do. His final pages, emphasizing determinism and precision as the 'good' in scientific psychology, will meet with general approval, but he has missed here his chance to point out how all science moves toward the abstract as it develops.

Some of my criticisms depend on personal opinions. More reflect what I think the evaluative consensus to be. Only a few could be called corrections. There is a great mass of information in this book. The index gives 669 names. Murphy, it seems to me, has written an exceptionally good book, as these undertakings in psychology go.

Harvard University EDWIN G. BORING

Experimentell-deskriptive Psychologie der Bewegungen, Konfiguration und Farben unter Verwendung des Flimmerphänomens. By KURT HAACK. Berlin, S. Karger, 1927. Pp. viii, 263, 1 pl.

This monograph presents the detailed results of a minute phenomenological examination of flicker. The discussion is presented under the three main headings indicated in the title, the first of which has appeared previously.

Haack devotes 28 pages at the beginning of his study to the exposition of his problem and to a thorough review of the literature dealing with flicker. Then, in the first section, he discusses the various forms of movement experience that he has found to appear in a flickering field. He distinguishes first between movement (Bewegtheit) and the object moved (Bewegten) and further, in the case of the former, between direction of movement (Bewegungsrichtung) and rate of movement (Bewegungsgeschwindigkeit). The basis for the first distinction is the observation that movement may be seen without a moving object being noted (p. 29). He remarks, however, that Wertheimer's description of phi must be modified since even in the absence of a perceived moving object the "Etwas" or the "Bewegung" "die Qualität Hell oder Dunkel oder Farbig aufwies" (p. 31). On the other hand, the carrier of movement was frequently visible for brief instants now in one position and now in another. Various forms of the experience were reported with rotary movement when the center was perceived as the center of a plane circle (p. 35) or of a sphere (p. 45), and when the center of rotation was imaginary (p. 61).

The perceived direction of movement might be in any one of the three planes of space—frontal, sagittal, or horizontal—and sometimes occurred at an angle to one of these (p. 48). The latter, however, seemed to be a summated or 'principal' direction in the presence of more than one possibility of direction. Some combinations, of course, were more frequent than others. Numerous factors in the stimulus and in the observer's attitudes modified the form and direction of the movement. A non-rotary movement is reported also and characterized as "Strömen" or "Schneeflockenfall" (p. 82).

Although flicker can not be separated from a certain impression of movement it is independent of the rate of that movement (p. 84). We must not forget that the movement experience Haack is discussing is the movement in a flickering field. It is only as such that he is concerned with it since flicker is intrinsically labile (p. 88).

Haack proceeds in the second section to describe the various configurations (Scheinfiguren und Scheinkörper) which appear in the experience of flicker as the carriers of movement. The familiar figure-ground concept is the basis of the discussion and the configurations of various elements—points, Schneeflockenstrom; lines, ziczaclinie, Haken; surfaces, Landkarte, geometrische Figuren—are described in the same detail as the aspects of movement have been, together with the effects of attitude, size, position, etc. upon the configuration.

In the final section, Haack reports the color phenomena that appear in the field of flicker. The configurations of the flickering field tend to be imbedded in a "Medium" (p. 189 f.). This impression is most frequently given in terms of brightness and is characterized as "Rauchwölkchen," "Flöckchen," "feiner, zarter Dunst," "ein dünnes Blättchen." The brightness of the "Medium" is perceived as illumination (Licht) which may be either light (Hell) or dark (Dunkel) but the latter are not simply of the qualitative series black-gray-white. "Das Hell und das Dunkel ist etwa so, als wenn es ohne Weiss bzw. ohne Schwarz wäre" (p. 192). Variations of brightness are qualitative as well as quantitative and require a system of terms, viz., dunkel-hell-lichtweisslich-graulich-schwärzlich. Colors (die bunten Farben) always have a different localization from gray. They either lie in front of the gray or are seen as though through a hole in it. Likewise their mode of appearance varies, —*e.g.* Tiefer und Breitenkohäsion und Adhäsion—and they come in such perceptual forms as shimmer, luster, glow, etc., and as one color seen through another.

We have in this monograph, then, a most exhaustive catalogue of what may be seen in flicker. Although the work is by title "experimental" a diagram of the apparatus is the only reference to the conditions under which the observations were made. While one cannot demand that a phenomenological study of this sort be given a statistical treatment, it is disappointing that no hint—unless it be the word 'flicker'—is present of how one may set up and observe the same phenomena. It is hard to escape the impression that the work though meticulous is casual.

Hobart College FOREST LEE DIMMICK

Changing Backgrounds in Religion and Ethics: A Metaphysical Meditation. By HERBERT WILDON CARR. New York, The Macmillan Co., 1927. Pp. 222.

The purpose of Professor Carr's "metaphysical meditation" is "to express definitely the consequences in ethics and religion of accepting the principle of evolution." He maintains "that all the phenomena of life belong to one evolutionary process" is a 'scientifically established' fact and that that is "the one fact in our scientific survey of the cosmos which offers us an analogy of what the universe in itself may be, the one fact which gives us the key to the construction of a metaphysics of existence."

Objective science discovers in nature two processes: "a process of cosmic evolution and a process of living evolution." Physics and chemistry deal with the first; biology with the second. The material of the first process is matter;

its principle, inertia; that of the second, life; its principle, activity. Cosmic evolution represents a descent. "To physics and chemistry the matter which constitutes the planetary mass which we are able to submit to scientific analysis is a stage in the degradation of energy." The direction of cosmic evolution is "from motion to quiescence, from activity to inertia, from instability to stability." The process of living evolution of life is an ascent. It is "continuous creation."

Now these scientific facts—matter and life—with their opposing principles, inertia and activity, show us only separate and one-sided pictures of the universe. Is there a possible way of reconciling them "as complementary aspects of one reality?" Physicists and biologists have "no clue to the relation between these two systems of objective phenomena, no interpretation of the opposing principles." Their primary concern is precise description of the facts in their respective domains. But "every scientific student is profoundly convinced that the universe is a rationally explicable unity." The question of the unity of the universe is not prompted by idle curiosity; "it is imposed on us by that appetition which is inherent in the nature of reason." Science rightly confines itself to the description of facts. Facts, however, must not only be described; they must also be interpreted and explained—but "to explain the universe requires metaphysics," therefore explanation "is the philosopher's job."

To explain the universe in view of the mentioned scientific facts means that metaphysics must "devise a rational scheme by which these two opposite principles, matter and life, shall be brought into unity in one concept," For dualism is not "satisfying to the scientific and philosophical conscience. In science and in philosophy mind apart from living body, living body apart from mind, are pure abstractions." To the biologist and psychologist the words 'life' and 'mind' denote forms of living activity and not independent entities.

The metaphysical principle that, in Professor Carr's opinion, can satisfactorily explain existence is life, viewed as creative evolution. It is the theory "that all actually existing living forms of activity are the present realization of a universal dynamic activity, manifesting itself as an urge or impulse or striving, not only one in its origin, but also one in the full extent of its multifarious process." This universal life-activity is actualized only in the individual; and it is only in individual experience that unity of existence can be found. Yet the individual seems to be of very small importance to the evolutionary creative principle revealing itself in him. His whole purpose seems to be "the carrying on of the life process beyond him"—in the creation of other individuals. After he has fulfilled this task he is cast aside. This indifference to the individual, on the part of the universal life activity, is found also in evolutionary religion and ethics.

It would be of considerable comfort to us if we could distinguish some purpose or end which the universal activity seemed to realize; but "there is no indication whatever, so far as the simple scientific facts are concerned, of a purpose or final end of living activity itself." Professor Carr's religious ideal of a perfected humanity, for instance, is not such an end. Our values, whether aesthetic, moral, or religious are not independent of us. They are not something outside or above the existing world; they are "the existing world in its

aspect to human experience." As to their future preservation the God of Creative Evolution cannot offer us any assurance. For this new God, supposed to be so superior to the old one, is the creative activity itself, for which "all human valuations are meaningless."

What, then, is the meaning of life when life is interpreted through Professor Carr's metaphysical principle. I believe the answer was happily formulated by the late George Simmel whose philosophy of life shows a similar orientation with regard to biology—"Die Tragik der geistigen Kultur."

University of Iowa BONNO TAPPER

Vom Problem des Rhythmus. By R. HÖNIGSWALD. Leipzig and Berlin, B. G. Teubner, 1926. Pp. viii, 89.

One of the most significant monographs on the concepts which underlie the rhythmic perception has appeared under the above title. Some graduate student well versed in technical German would do us all a service by translating it. The author makes no apologies for establishing his base line in philosophy in order to interpret more adequately the experimental results obtained in the psychological laboratories. Not only does he offer us, therefore, an analytical study of the concept but he relates it very ingeniously to a number of outstanding systematic points of view such as those of *Gestalt* and *Struktur*. At the same time the interrelation of the actual experience of rhythm as a mental reality and the theory of its interpretation is assumed.

The author clears the ground by telling us what he is not going to do. Then he proceeds to erect his own idea of rhythm as a psychological reality. Without doubt rhythm is fundamentally a *temporal* experience which is cognitively differentiated and unified. It is not a single membership of elements but an organization of members. But while it presumes the temporal aspect it is not ultimately defined in terms of this principle because time as such can be experienced as an entity only in relation to other experiences orientated in time. The experience of articulated time combines with the articulated experience of time and becomes the ultimate background of rhythm. This fundamental fact next suggests the observation that rhythm can be scientifically envisaged only through a mentally produced objective relationship. Here again we find the inevitable paradox that from the dynamic point of view rhythm must take its place among other cognitions as an act of perception as well as a referable circumstance in the environment. Subjective and objective are melted together into one alloy. Rhythm is, therefore, founded on the relation between experience as such and that which is experienced.

This leads us at once to the next important consideration that rhythm is not a sum of elements but a whole. It is a product which though mentally experienced and organized appears to be objectively conditioned. From this point the author weaves his concept through various circumstances and interpretations. Sensation and stimulus, psychology and physics, mental activity and objective conditions are compared and differentiated. In passing, a number of concepts are interpreted such as, configuration, partial configuration, and special configuration. The problem of empty time is also faced. Not only are there sallies into present day points of view but even Leibnitz is made to

contribute to the concept of 'monadity' which, as far as the reviewer can gather, is an expression symbolizing an abbreviated cognition of a kaleidoscopic world as it is mentally unfolded from within. In this wise especially in rhythmical performances is the cognitive element also unfolded, ultimately bringing rhythm into a close approximation to the *Struktur* or personal self. The importance of characteristic personal rhythms is emphasized not only in the industrial world but also within the latent habits of association and thought. The last touch is given to rhythm in terms of memory and recall. When it is finally delineated it emerges as an entity which requires reproduction for its existence.

Thus we are brought to the end of a monograph which provides us with one of the keenest analyses on the theoretical level which we have had thus far. It symbolizes also a type of theorizing which is not very common in this country although there is some hope for it since physics has furnished us so good a precept as well as an example. For, according to the author, philosophy is in the broadest sense the underlying theory of objective conditions while psychology in a narrow sense is simply a phase of this theory. The monograph shows a lively style, deep insight, broad scholarly orientation, as well as vertical thinking!

University of Iowa CHRISTIAN A. RUCKMICK

Zur Psychologie der Tiere und Menschen. By W. BETZ. Leipzig, J. A. Barth, 1927. Pp. xi, 206.

In this delightfully written treatise—completed by the author's confession in ten weeks—we have a running account of mental development from the lowest forms of animal life to the highest, including man. The author was interested in developing his psychology because he soon became convinced of the relative unimportance of consciousness. He makes much of the fact that there is a considerable amount of 'intelligent' activity without consciousness, that a large portion of the brain is anaesthetic and that the reflex adjustments of the body to rotation and position are not found in experience. Even learning in the conditioned reflex occurs without conscious effort. This develops into the curious situation that one can tell, in many instances, what one thinks but not how. When the author says that a person can be awake but not conscious our opinion is confirmed that he is using the term conscious in the sense of reflective experience and does not admit the lowest forms of cognition.

The chapters are replete with suggestive material most of which betrays itself of an origin which is nevertheless stimulating. Perception, sensation and feeling, general orientation, recognition, concept, idea, social phenomena, conditioned reflex, instinct, learning, and the thought processes are included in the chapter headings and give us some idea of the comprehensiveness of treatment. These comprise only one half of the treatise. The other headings cover such subjects as speech, the variability of instincts, and intelligence and mind. They suggest applications that have been worked out through the whole animal series. Throughout the work there are many peculiar analogies. The Chinese pagoda, for example, is supposed to have obtained its characteristic shape originally from the trees which our progenitors, the anthropoid ape, knew so well.

It is a matter of regret to the author that man has attained so large a size. We could be so much better provided with food if we were the size of cats or even of mice. The only difficulty would be the unevenness of combat with large animals, but chemists among these diminutive men could devise odors and gases which would make the world safe for mice-men. There is much organic development which is profitless and impractical for even the brain could be much reduced in size.

Many of the analogies are ingenious. The difference between our inherent reaction to visual objects as such and our responses to a world of pictures is naturally brought out by the device of going about looking at our environment through a frame of ground glass as in a camera. In the last chapter intelligence is reduced to a reflex mechanism which reacts to situations as a whole and in the rough. The only new qualifications are adroitness of response, elasticity in adjustment and refinement of adaptation to environment. But he holds no great hope for the improvement of intelligence over the next few millenniums. One must be satisfied if it does not essentially deteriorate.

The book can not be taken as a scholarly treatise but merely as a survey. Many of its conclusions may be answered or doubted but most of them have a certain plausibility when framed within the compass of their specific application. The range of treatment is enormous. A typical instance of the sweep of the imagination based upon fact which is displayed in the book is illustrated by the observation that it is probably impossible for fish to see spatial objects yet man can turn space into time in the theory of relativity and he can transplant geometry into algebra!

University of Iowa CHRISTIAN A. RUCKMICK

Histoire de la philosophie. By ÉMILE BRÉHIER. Paris, Félix Alcan, 1926-28. Vol I, pp. 791.

This work, of which only the first three instalments have yet appeared, represents an attempt to approach the problem of the history of philosophy with a minimum of assumption concerning its origin, its autonomy, and its progressive development. The first instalment covers the period from Ionian natural philosophy to Aristotle and the Lyceum; the second continues the exposition to Neoplatonism and the intermingling of Hellenic and Oriental ideas at the dawn of the Christian era, while the third, which is devoted to mediaevalism and the Renaissance, brings the discussion down to the end of the sixteenth century and completes Volume I.

While recognizing with Hegel and Comte (in contrast with an eclecticism or an unfruitful scepticism which finds historically only the strife of competing sects and systems) that there is indeed a unity of the human spirit which links the past to the present, Bréhier purposes to explore the material presented chronologically in order to ascertain if possible what the law of connection may be, rather than to exploit the data for the illustration of any preconceived law. His investigation proceeds accordingly on the basis of minute special studies of the various authors and epochs and hence is furnished for reference with an adequate supply of scholarly footnotes together with a well-selected working bibliography for each chapter. This plan is carried out consistently

throughout the work with a thoroughness which can hardly be indicated in a merely preliminary review.

One is continuously impressed by the penetration of the author into the central meaning of a given doctrine which, with his sure grasp of details, enables him thoroughly to organize his material with a welcome freshness of treatment where a merely cut and dried recital of doctrine would not be unusual. A reader would, for example, hardly look for more than a stereotyped account of the fragmentary teaching of pre-Socratic philosophers, and yet Bréhier reinterprets them all in a highly interesting manner, illuminating even some of the obscurer utterances of the "gloomy Heraclitus." The volume is in general the product of conscientious, mature scholarship and, without descending to the plane of a first seller, is readable by anybody who wishes to read a book on this subject.

Vanderbilt University HERBERT SANBORN

Cours de morale a l'usage des écoles primaires supérieures et des cours complementaires (IRE année). By FÉCILIEN CHALLAYE and MARGUERITE REYNIER. Paris, Félix Alcan, 1927. Pp. viii, 232.

As life comes from life so, we are informed in the preface, the moral life in every individual is born of the moral life of those who have preceded him. Man can no more make his own morality than his geometry. The great moral leaders of all ages—Buddha, Confucius, Socrates, the prophets of Israel, Jesus, Luther, Rousseau, Tolstoi, etc.—take and emphasize some large idea more or less directly from tradition in their own social milieu, and they make it valuable for mankind of all countries and of all time. One then, oriented toward the future, confronts his own personal experiences with ideals which he has received from society. Thus, we read, the moral life "ought to be the product of social traditions and of individual experiences and reflections." The two virtues which the authors think ought to be the most highly esteemed by future humanity are love of work and fraternal goodness or kindness (*bonté fraternelle*). The former of these "imposes the duty of exercising an indisputably useful profession, of honoring all workers, and of demanding for all of them a situation materially and morally satisfying. The maximum of well-being and culture." "Fraternal goodness, condemning violence in relations among men and among different classes, orients all conscious endeavor towards an ideal of harmonious humanity."

Thus from a good beginning in the preface the authors run more and more into general formulas as to duty, and later they go into sentimentality, as is probably natural to do with youth. They "expound this code with optimism and confidence in human nature." Convinced that morality is a matter of general sentiment rather than of cold logic, (it is characteristic not to see any middle ground) they aim less to demonstrate than to motivate. The course which they outline for the children and adolescents consists of an introduction on the superiority of man over animals and on the good will conscientiously striving for accomplishments in the way of just acts; twelve chapters on individual life—courage, love of work, love of truth, sincerity, modesty, etc.; four on the family—duties to parents and to various members; two on the school—duties

to the teachers, respect for rules, amity and comradeship; two on professional duties—choice of a profession and the professional consciousness; and eight on society, along the same line—generosity, respect, politeness, etc. The lessons consist of short, usually sentimental, expositions followed by résumés, 'thoughts,' and 'readings' from famous poets and other great men, and questions for personal work. It is the Sunday-school type of course, and is as blind as such courses usually are to the inadequacy of mere formal exhortation and of the arousal of sentiments about right doing, and as disregardful of the means of attaining the desired well-being of all. This is indoctrination rather than education. It would be interesting to obtain by some reliable means the actual effects upon moral conduct of such instruction, much of which is mere propaganda. Some very good selections are included in the *Cours*.

J. P.

L'orientation lointaine et la reconnaissance des lieux. By ÉTIENNE RABAUD. Paris, Félix Alcan, 1927. Pp. 112.

The author assimilates into a coherent whole the facts so far obtained by himself and by other investigators on the means various animals have of regaining their nests. The method of attacking the problem was threefold: observation in a natural setting where the investigator remains passive, experimentation in a natural setting where the investigator may partially control the conditions, or experimentation in the laboratory where the total situation is controlled.

Rabaud is emphatic in dispelling any interpretation that hints at a mystical sense of orientation through a means not known to man or not consistent with the structure of the animal nervous system.

The predominance of particular sensory cues employed by different animals depends on the type of organism or on the particular circumstances within a particular type. Bees use visual cues to bring them to the immediate environs of their hive while olfaction guides them within the hive. Ants that travel in columns are stimulated by the odor of the trails they have made. Solitary ants use vision. Molluscs resort almost exclusively to tactual cues. The discussion of the orientation among vertebrates is brief, being limited chiefly to pigeons. Here vision plays the largest rôle with the secondary support of kinaesthesis.

The behavior of invertebrates and vertebrates is fairly similar. They both employ various sensory cues that must be in sequence but not necessarily in a chainlike order as the suppression of a particular cue does not inhibit the return to the nest. The orientation in all cases seems to be configurational. All cues have some bearing on the total situation. The nest also is not an isolated landmark but belongs to a total setting.

Two points of interpretation by the author cannot be accepted, the one that images of various sensory cues are registered within the nervous system, and the other that these images serve in a memorial function.

One admirable feature of the book, however, is the tendency of the author to realize that the environment of a particular animal is peculiar to him and need not be at all the setting as we perceive it.

Sarah Lawrence College THEODORA MEAD ABEL

Reconstructing Behavior in Youth. By WILLIAM HEALY, AUGUSTA F. BRONNER, EDITH M. H. BAYLOR, and J. PRENTICE MURPHY. New York and London, Alfred A. Knopf, 1929. Pp. xi, 325, ix.

Any academic psychologist who becomes discouraged by the baffling nature of the problems which he faces should read this book for spiritual refreshment. He will soon discover that the difficulties which confront him in his efforts to devise apparatus, construct satisfactorily valid and reliable tests, or even to keep up with all of the new departures in statistical procedure are insignificant and trifling in comparison with the apparently insuperable obstacles which the field worker of the social agency confronts who seeks to utilize the world's supply of psychology in the reconstruction of delinquent youth. Here is the story of what was done with 501 cases, and what came of it all. The different kinds of delinquencies are segregated. What was done with them is told along with reports and summaries of successes and failures. There are chapters on the technique of child placing, on the selection of foster parents, their training and their supervision, and on many other such matters. The appendix presents two cases in complete detail and a full statistical summary. There is wisely no attempt at psychological generalization; and yet one cannot read these cases, with the tales of superbly tactful handling along with the occasional frank admission of blunders, without a feeling that much progress is being made. There is properly no psychological bias, but an evident desire to utilize every possible scrap of psychological information, whatever be the source, which may in any way help in the solution of the problems.

One is inclined to be very generous in commenting on such a report—too much so, perhaps. It certainly ought to be of service and a source of inspiration to many. At times, as he read, the reviewer wished that the book might have been written by one hand instead of by so many. It was to that multiple authorship that he attributed the repetitions and what seemed at times a failure to achieve an agreeable smoothness and ease of organization and presentation.

University of Oregon EDMUND S. CONKLIN

The Study of Religion in State Universities. By HERBERT LEON SEARLES. University of Iowa, Iowa City, Iowa. University of Iowa Studies, First Series No. 141. *Studies in Character*, Vol. I, No. 3, 1927, pp. 91.

This study presents itself in two divisions: (1) the presentation of material to prove the highly dubious point that there is, in its incipiency, a science of religion; and (2) the collation of matter to prove that the former impossibility of teaching religion in public schools is surely resolving itself into a possibility and a desirability. Under the first division, and in conformity with Karl Pearson's definition of scientific method, it is alleged that there is a science of religion consisting of fact-gathering and fact-arranging, deductions of hypotheses from arranged facts, and *disinterested* inquiry. These three features of methodology would make use of the history of religions, the comparison of religions, the philosophy of religion, and the psychology of religion (if any such exists over and above the subject-matter contained in the other three approaches). The method would cull much from literature, archaeology, anthropology and ethnology, philosophy, and psychology.

The history of the status of the teaching of religion in public schools in this country is given at length. Constitutional and other legal sanctions and prohibitions in the various states are presented. The present status in colleges and universities, together with growing changes in it in the form of *trends*, is pointed out. Some thirty-seven state universities, or their adjuncts or subdivisions, it appears, are engaged in this type of work, in one form or another. The thesis of the study is: if all knowledge is the domain of the university or college, it cannot fail to encompass that portion of knowledge which relates to religion.

The paper contains, besides Foreword and Acknowledgments, eight chapters, eighty-one references, and four appendices. The appendices relate respectively to which state universities engage in the work; questionary data from teachers as to the advisability of such teaching, difficulties met with, etc.; constitutional and other legal restrictions upon such teaching in the various states; and the plans now being followed or being formulated for use in the University of Illinois, Kansas School of Religion, Ohio Union School of Religion, University of Oklahoma, Wesley College at the University of North Dakota, Bible College of Missouri, Michigan School of Religion, and the School of Religion in the University of Iowa.

The whole monograph creates in the mind of the reviewer the suspicion of widespread, but none the less dignified and pious, propaganda, put forth by zealots and politicians, within and outside of college faculties and administrations, to cater to forces which seek to preserve from a natural and well-deserved death the moribund and decaying ecclesiasticism and credistic denominationalism in which most of us grew up.

University of Oregon H. R. CROSLAND

Mouvement et pensée. By M. G. PEUCESCO. Introduction by Charles Richet. Paris, Librairie Félix Alcan, 1927. Pp. 170.

In this simple presentation we have an attempt to explain the highest mental processes of thought as an outgrowth of the reflex arc. When the reflex is inhibited and the refractory period is interposed an interlacing of reflex arcs occurs whose output is a resultant similiar to the total reflex action. These inhibitions together with the refractory period are due to the vibrations which counteract one another producing oscillatory movements in the cells of the brain. Thought is, therefore, reduced to a vibratory movement but of a somewhat mysterious sort. While the work aims to be grounded in physiological psychology and avoids altogether a psychological animism the sources and movements described have more of a biological than a physiological connotation.

There are several schemes illustrating the convergence of neural paths and their resultant in terms of a single form of energy. In the final chapter the author pleads for an integration of the mental life based upon this interplay of vibratory movements. The highest form of reasoning then is derived from a mutual interference of neural currents illustrated on the lower plane in terms of motor inhibitions and the refractory period.

While the author cites histological data and physiological researches from numerous investigators the general tone of the book is theoretical.

University of Iowa CHRISTIAN A. RUCKMICK

Die Gefühlswerte: Grundriss einer Psychologie der Tiefe. By F. Noltenius. Leipzig, J. H. Barth, 1927. Pp. 352.

In this book the author presents his conception of the nature of psychology and especially the significance of affective states in the development of the various systems of evaluation regarded as a basis for social control. There is an analysis of psychological problems on the basis of their theoretical importance, and in this the point of view is genetic and social. In addition to a critical analysis of the term 'mind' (*Seele*) there is a historical account of the way in which this term has been modified in its usages in medicine, art, ethics, logic, and religion. Through all these chapters the author makes application of his conception of the nature of the affective processes. From the more intensive affective processes of primitive man, the author passes into their relationships to modern conceptions of value and the cultural processes upon which they are based. There is a sharp discrimination between the good and the true as resultants of affective processes, and particularly as derived from the series of which unity and totality may be regarded as limits. The technique which the author uses corresponds in some respects with psychoanalysis, but the deviations must be considered before attempting a critical evaluation of the method.

The description of the abnormal phases of psychology, while not based upon the results of careful analysis or experiment, are of interest to those working in the field of the psychology of religion. For the American reader the book will be regarded as polemical rather than scientific.

Ohio State University — Albert P. Weiss

Angewandte Psychologie. By Emil Abderhalden. Berlin, Urban & Schwarzberg, 1927. From Abderhalden's *Handbuch d. biol. Arbeitsmethoden, VI. Methoden d. exper. Psychol.*, C, II, 1-744.

This number of the Handbook of Biological Methods is devoted to applied psychology. It represents one of the most extensive contributions to this field. It is restricted mainly to the German industries and is made up of and describes carefully the various tests, the apparatus, and the instructions and test blanks that have been used in the various investigations. No attempt can be made in this review to list the many different studies that are reported. All phases of applied psychology are treated, and it would seem that the workers in this field could not afford to overlook this volume of the handbook.

Ohio State University — Albert P. Weiss

Your Growing Child. By Bruce H. Addington. New York, Funk & Wagnalls Co., 1927. Pp. 405, ix.

This is one of the ever-increasing number of books written for parents. The book is written informally rather than in the more technical style of the scientist. The importance of early training in the home is emphasized in contrast to the influence of later training received in school. Throughout the book the influence of environmental factors is stressed and that of heredity minimized. The book is not intended for the student of psychology and consequently is of little value to him.

Peabody College — S. C. Garrison

Experiments in General Psychology. By NORMA V. SCHEIDEMANN. Chicago, Univ. of Chicago Press, 1929. Pp. 108.

The book is intended not as a laboratory manual for experimental psychology, but it purposes to "give the student an opportunity to experience, in a systematic manner, various phases of mental life." The 52 experiments, illustrative of parallel textbook material, cover sensory phenomena, imagination, illusion, emotion, attention, memory, learning, association, suggestion, dreams, personality, and reasoning. The apparatus required is extremely untechnical and can be readily improvised by the student. Directions for procedure and space for tabulating, recording or interpreting results are given for every experiment. Much of the material presented in this notebook manual would be superfluous in a course where there are classroom demonstrations; but it may be used as a guide to experiments which can be performed individually. It includes, however, much that is trivial and scarcely on a level with usual experimental procedure. It is to be regretted, also, that only one experiment is devoted to auditory phenomena. A collection of the common visual illusions with questions and explanations in terms of perceptive cues is a good feature of the book.

Florida State College for Women — EMELINE MOUL

Manual of Psychology. By G. F. STOUT. Fourth ed., revised, in collaboration with C. A. Mace. London, University Tutorial Press, 1929. Pp. xix, 680.

This work is sufficiently well-known to require little comment by the reviewer, especially since no fundamental changes are introduced in the new edition. Professor Stout is aptly referred to, in the preface written by Mace, as "a representative of the central tradition in British Psychology." He is not an experimentalist, and there is naturally a tendency to settle problems by verbal formulations which often are tautological. For example, his explanation of the learning process by calling it a "cumulative disposition" means no more than "learning is learning."

The preface to the first edition refers to the book as an "exposition of Psychology from the genetic point of view." The approach is genetic in the sense that the leading topics are discussed in the order: sensation, perception, ideation.

There are extensive discussions of methodology and of the mind-body problem. The book is very comprehensive, and is perhaps too difficult for use as an introductory text, although the author refers to it as such.

Vanderbilt University — LYLE H. LANIER

Freiheit, Wollen, Aktivität. H. REINER. Halle, Max Niemeyer, 1927. Pp. 172.

This work deals with the phenomenological investigations directed toward the problem of free will. The problem of freedom itself is critically discussed, especially in its relation to the problems of human adjustment and its manifold manifestations. The relationship between freedom and volition and resulting activity is given especial attention. This is a contribution toward a systematic psychology.

Ohio State University — ALBERT P. WEISS